THE
POPULAR SCIENCE
MONTHLY.

JULY, 1904.

# A VISIT TO THE JAPANESE ZOOLOGICAL STATION AT MISAKI.

By Professor BASHFORD DEAN,
COLUMBIA UNIVERSITY.

JAPAN is not at its best in the rainy season. For the rain comes down in floods. And my first impression was that Misaki was a larger aquarium than even a zealous naturalist needed. We had left Tokyo at six—I was about to say, early one morning, but I recall that six is not early in Japan—on a small bay steamer which plies daily to Misaki. And a few hours later we had about reached a climax in our rolling, when, turning suddenly, we ran under the lee of an island and came to anchor. I confess that I was not cheered by the glimpse of Misaki; the town was a flat, sodden mass of thatched houses, its background an abrupt knoll, with a ragged skyline of dripping and irregular pines, and the drooping eaves of a temple. And to add to the dismalness of the picture, even the sampan men appeared tearful as they shedded streams of rain from the points of their porcupine-like coats. Our fellow passengers, on the other hand, showed not a symptom of discomfort, and they clambered smilingly into the sampans, standing or crouching under a mass of oil-paper umbrellas, men often tucking up their kimonos and standing bare-legged like storm-bound birds—their wide wooden clogs giving them the appropriate webbed feet. Ashore was waiting for us an assistant of the station, Mr. Tsuchida, and together we waded through the narrow and fishy streets of the town (which I found, to my surprise, had a population of five thousand, and was of no little commercial importance in furnishing fish for the Tokyo market) to the Inn Kinokuniya. This inn I recall vividly, for its host, in spite of the drenching rain, thought it necessary to hunt the town for a knife and fork for the foreigner, and while

about it borrowed a chair and table, probably from the station house and its solitary policeman, and provided 'beefu teki' and 'pan' (bread), in order to make things homelike, as he said. And while he busied us with these incidentals, he engaged a sampan to carry us to the station. I might mention that in *real* Japan the traveler can, or should, do little without the aid of his innkeeper—if one wishes to go to the railroad, a theater, a shop, or to hire a boat, a coolie, a jinrick-

THE MISAKI ZOOLOGICAL STATION.

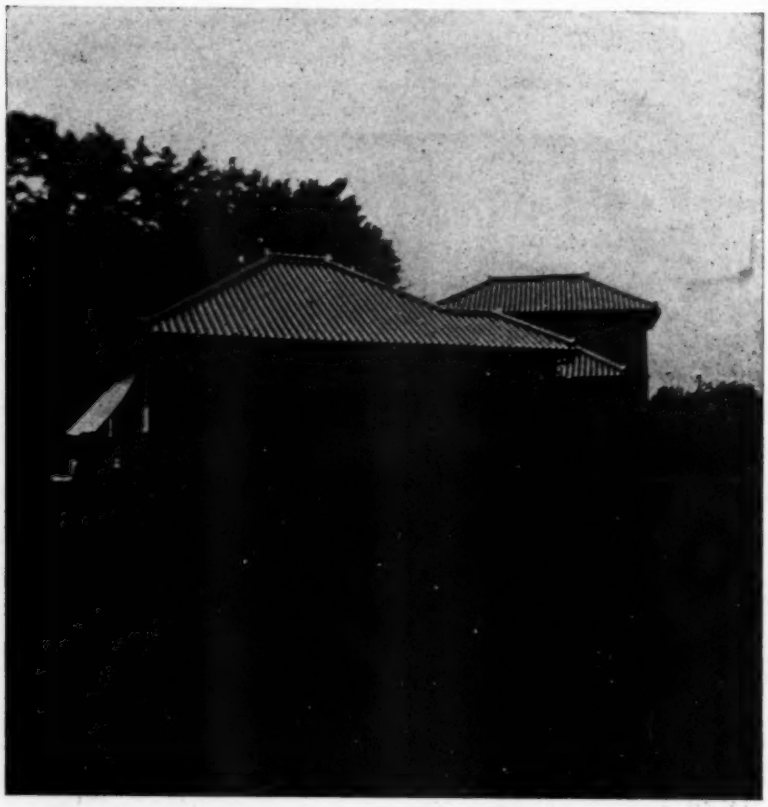
shaw—it is *de rigeur* to go first to the ever-present inn. I soon discovered that our host was on excellent terms with the zoological people, for the station had formerly been located near by in the town. But the town was found to be not the best of locations; there was too much noise, and—fish market, for example—so the building was moved bodily around the point to a small rugged peninsula which forms the harbor of Aburatsubo, about a mile away. Presently our host shelled us in rain coats and deposited us in our sampan, and our ferryman, sculling with a heavy balanced oar, shot us beyond the island, whose lighthouse guides the steamers into the mouth of the bay of Tokyo, and then, turning sharply, he skirted the coast, around the edge of the sea of

Sagami. At this point of the trip I recall that some confusion was created, as we rounded a swell-swept rock, by our sailor falling overboard, his oar becoming suddenly unshipped, an incident remembered mainly on account of the poor fellow's embarrassment. We pulled him into the boat and reinstated him; and he shook off his dripping kimono and stood naked in the drenching rain; bronze body, white loin cloth, white band knotted around his forehead, pressing down a fringe of bristling black hair, his muscles showing splendidly as he swayed at his oar, hissing viciously as he pushed and pulled. In a few minutes more we rounded a little pine-covered point, and the two white buildings of the laboratory came into view.

BEACH OF THE CASTLE OF ARAI, SEEN FROM THE STATION.

The taller of these, two-storied, is the one which was removed from the town of Misaki in 1897, the other was built a couple of years later. Together they stand close to the water, but are sheltered from typhoons by an abrupt hill which forms the end of the point. The surroundings are beautiful. A number of inlets cut deeply and irregularly into pine-covered hills, and in nearly every direction one obtains vistas of

bluffs, pines and rocks very much as at famous Matsushima. Altogether I believe that the student here enjoys more picturesque natural surroundings than at any other laboratory in the world. And we may add to this the unzoological item that the headland has a romantic background. For here was the castle of Arai, famed in Japanese history as having withstood for several years the siege of the Hojo regents during the fourteenth century: and on every hand are memories of its past glory.



THE MARINE STATION AT MISAKI.

If I digress a bit, I might point out that the student dormitory, amid the old pines on the hilltop above the laboratory, and next to Professor Mitsukuri's villa, is built on the exact site of the ancient castle, and here interesting relics have been found; such, for example, was a fragment of a splendid gold-crested helmet dug up during my stay. Near by are traces of fortifications, and a store-room excavated in the rocky bluff during the ancient days of the castle. The bay, at the side of the laboratory, is still called the 'Red Harbor,' because at

its very edge the defenders were beheaded after the sack of the castle. And just opposite is the 'Buried Treasure-Beach,' where the most valuable plunder of the castle is supposed to have been stored. And more interesting still are the monuments marking the spots where died by hara-kiri the lord of the castle, Dōsun Miura, and his son, after word had been brought them that all hope was lost. The local story is that Dōsun Miura retired to this point after witnessing the death of his son, and fearful lest his own head should be carried across the bay to Odawara by the conquerors, he would trust no one to act as his second in the death ceremony. Seizing his short cue with one hand, *he is said to have cut off his own head with the other, and to have thrown it far out in the deep water before his body fell,* a physiological possibility, by the way, which students of the laboratory do not question—in the presence of townspeople. So the greatness of the Miura is unimpaired, and every year memorial services are celebrated on the laboratory grounds. At this time portrait-images of Dōsun and his son are brought from a neighboring temple and placed on the altar in a prayer-tent near the beach, and wrestling bouts commemorate the siege and the fall of the castle. Perhaps I might end my digression with the note that the present property came into the hands of the imperial family and has remained unoccupied since the fifteenth century. The major reason for this is said to have been that the point was haunted and many curious stories are told of the reappearance of Dōsun Miura and his men on the hilltops among the ancient pines. One recognizes them readily, since, like all Japanese specters, *they have no feet.* Indeed, I learned through Mr. Alan Owston, of Yokohama, that even a few years ago, when his yacht anchored overnight at Aburatsubo, the point was still so ghost-ridden that the sailors were unwilling to go ashore!

As for the zoological station itself: The one-story building is used by the graduate students, and is divided off in alcoves in the usual way. Work places for eight investigators are provided on the north side of the main room. On the south side of the building is the office of the director, and forming the fourth corner of the building is a concreted room containing aquaria and giving ample space for the preparation of larger material. By a covered way one passes from the door of the advanced laboratory into the two-story building, the ground floor of which is used at the present time for general class work. The upper story contains two living rooms fitted in European style, which were generously placed at the writer's disposal by the authorities of the Zoological Institute.

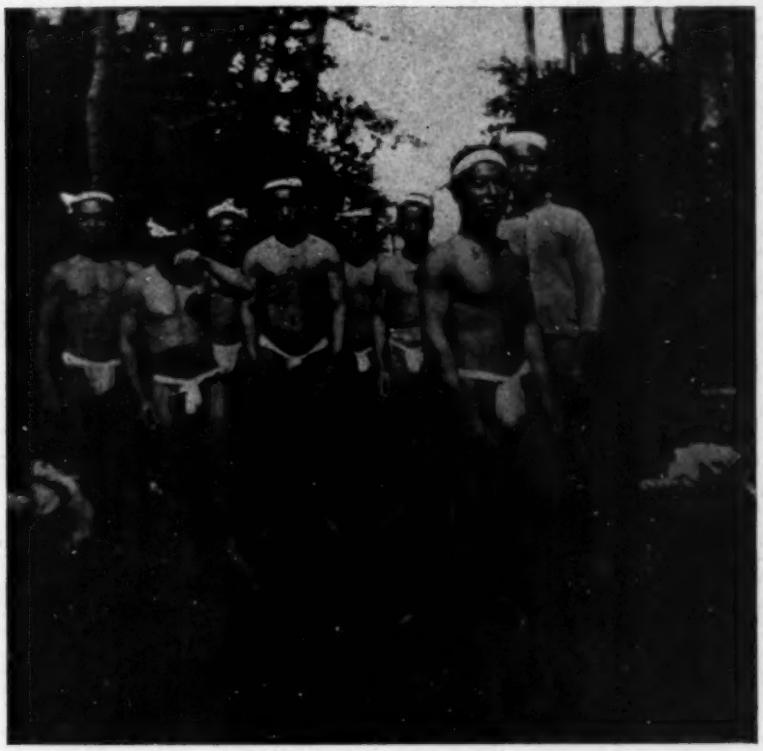
The general class work consists of a summer course of about six weeks' duration, which gives its members an opportunity of becoming familiar with the structure of the prominent animal types. The stu-

'ARAI,' THE COLLECTING BOAT OF THE STATION.

dents, about thirty in number, are usually teachers of zoology in various high schools throughout Japan. Some of them live in the dormitory on the hilltop, others find lodgings at the neighboring fishing village of Aburatsubo, often hiring quarters in the little temple on the top of

A FISHING BOAT.

the hill. The investigators include Professor Mitsukuri, one of the founders of the station and its director, and usually the greater number of the staff of the zoological department of the Science College. Professor Ijima frequently comes down to visit his reefs of glassy sponges, Professor Watasé, Professor Goto, Dr. Izuka, Dr. Miyajima, Mr. Namiyé, some of the younger assistants, and three or four of the graduate students of the institute at Tokyo make up the remaining corps of investigators. During the writer's visit, a Russian ichthyolo-



A Crew of Fishermen.

gist, Professor P. Schmidt, became a guest of the station, and an American zoologist, Mr. J. F. Abbott.

The work-quarters, it may be mentioned, are simple, but all necessary appliances and books are promptly forwarded from the institute at Tokyo. It is the collecting facilities which the visitor does not forget, for not only is the locality a rich one, but the ways and means are at hand to secure material even from great depths. And in this lies the value of neighboring Misaki, for during many months of the year the fisher people set out at sunrise in their large boats, proceed

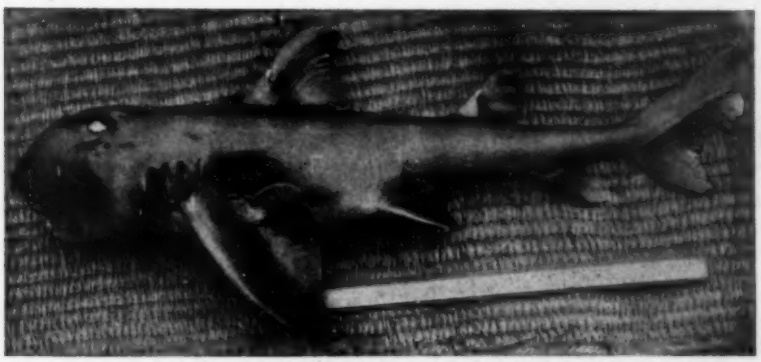
off shore to well-known fishing reefs, which, by the way, are often in very deep water, and return during the afternoon with a varied haul. If successful, their home-coming is spectacular—they chant in chorus and push their heavy boat through the water, sometimes skulled by as many as a dozen oars, at the rate of a young steam launch, the boat garnished at the prow, and the crew wearing fillets and loin cloths of scarlet. It happens, fortunately, that the fishery is carried on principally by hand trawls, for it is clear that when such a line, which is sometimes a mile in length and with thousands of dependent hooks, is pulled up again, even if fish are not taken, there will surely be entangled a varied collection of objects—sponges, echinoderms and rock fragments, the last often richly stocked with brachiopods, worm tubes, corals, bryozoa. Happily, too, the collector of the station, Kuma Aoki, is an ex-fisherman, for, knowing the townspeople, he serves as a diplomat, suggesting regions which should be fished, and often accompanying the expeditions. To be mentioned in this connection is the skill with which the fisher people are able to locate accurately fishing grounds. By the use of a system of cross ranges, a master of the craft like Kuma can return to a spot where he has lost a valuable fishing line, and can secure it on a following day—a result which seems the more remarkable to the novice when he reflects that the line may have been lost in 400 fathoms of water.

While the trawl line is the customary apparatus of the fishermen, numerous devices are also employed in special fisheries, an account of which has been given recently by a Russian ichthyologist (cf. Dr. P. Schmidt, *MT. d. Deutschen Seefischerei-Vereins,* No. 2, p. 31, 1903). I might mention particularly the use of earthenware urns which are fastened together by straw rope, and sunken in the coves in the neighborhood of the station. These are constantly used by octopus as places of retreat during bright daylight, and to secure them the urns have merely to be overhauled from time to time. Shell fish are often taken in the usual eastern way by the use of a water glass and a dart-pointed bamboo pole, or, without a water glass, the fisherman may simply thrust his head below the surface of the water. Especially useful to the collector are the numerous divers of Misaki, who are, I may add, so skilful that they use no apparatus, not even weights for rapid descent, but will swim down duck fashion, to a depth of twenty or thirty feet. They hunt especially *Haliotis,* examining the rocks deliberately, and often remaining below several minutes. I may mention that one of the familiar sounds which one hears when rowing in the neighborhood of the station is the diver's peculiar whistle, by which he expands his lungs before descending.

One need hardly review the fauna in the region of Misaki. It is enough, perhaps, to say that here focus many favorable conditions for

A Living Hag-fish, Showing a Mass of Freshly Secreted Slime.

marine collecting. There is a rich shallow water fauna which yields, among other delectable things, *Amphioxus,* sea-pens, a giant *Balanoglossus, Onchidium* and a *Phoronis* four inches in length. In the immediate neighborhood of the station can be obtained at low tide *Antedon,* very abundant, *Lingula,* the spawning of which Mr. Yatsu has observed in the laboratory, and *Cœloplana,* the latter discovered by Mr. Abbott. Pelagic forms can also be collected favorably. *Pyrosoma, Appendicularia* and *Salpa* are abundant. Various ctenophores, including *Cestus,* are not uncommon, and near the station, sometimes sweeping close to the shore, is the Black Current, bringing many southern forms. I may mention that the dead shells of *Nautilus* have been



A Freshly Caught Port Jackson Shark.

picked up near the station. The richness of the neighborhood in deep-water forms has been well known since the studies of the *Challenger*. Glass sponges of many species are frequent prizes of the fishermen. And stalked crinoids (*Metacrinus rotundus*) are often taken off the reef Okinosé. Among the fishes *Bathythrissa*, a primitive deep-water teleost, of which the *Challenger* was able to obtain but few examples, is now taken off Misaki so abundantly that it is regularly shipped to the fish-market in Tokyo. Hag-fishes are common, even more than common, and there have been collected within a relatively small area three genera and four species. Among the sharks *Heptanchus* is common; *Mitsukurina*, which is perhaps the Cretaceous broad-nosed *Scaphanorhynchus*, is taken occasionally. A Port Jackson shark is abundant, and in the course of a year the neighborhood yields about a dozen specimens of the frilled shark, *Chlamydoselachus*. Of chimæroids, *Chimæra phantasma* is common, and *C. mitsukurii* and *C. purpurescens* also occur; rare, however, is the long-nosed chimæra, *Rhinochimæra pacifica*.

One need hardly remark that the possibilities of Misaki are not exhausted in producing new and extraordinary forms. To cite merely an instance of this, during the writer's visit two hag-fishes were obtained, one of which, *Paramyxine atami*, was transitional between Myxine and Bdellostoma—its outer gill openings being drawn together within the length of about a centimeter; another (*B. okinoseana*) was transitional between the hag-fishes of many and of few gills, a large form with eight gill-openings on either side. In fact, it is coming to be expected that each year is to bring to the Zoological Institute at Tokyo prizes from Misaki—one year new forms of sponges; another, a gigantic tubularian hydroid, *Branchiocerianthus*, as a 'gift from the sea goddess Otohimé'; and another, specimens of umbrella-shaped octopods, *Amphitretus*. Under such circumstances it is not unnatural that the visitor should bring away from Misaki a stronger impression of his individual work in collecting—and this implies a clearer picture of the local fauna—than from many an older and better known zoological station. And he might justly add that the friendship of his colleagues of Japan is not the least enduring memory of his stay.

# HUGO DE VRIES'S THEORY OF MUTATIONS.*

By Professor A. A. W. HUBRECHT,
UNIVERSITY OF UTRECHT.

THE theory of evolution has influenced human thought in the most various ways during the past half century. In the sphere of biological science, where Darwin sowed the seeds which have grown up with such unexpected luxuriance, there has been a continuous process of fermentation which shows no signs of subsiding.

Still it has often seemed as if the immense mass of facts, which Darwin collected and arranged with so much care and skill, were provisionally looked upon as sufficient, and as if actual experiment were no longer a primary necessity. Whenever a new observation happened to be made, it was in danger of being drowned in pailfuls of theoretical considerations. Genealogical trees were planted, grafted, transplanted and finally often committed to the flames. Statistics were brought together to demonstrate the importance of natural selection, not only in the struggle between individual organisms, but also within the organism between the elements of which it was composed. Thus Roux wrote in 1881 his 'Kampf der Theile im Organismus,' Weismann only a few years ago (1896) his 'Germinal Selection.' In a series of very remarkable publications the Freiburg professor of zoology has thrown light on a series of difficult problems and has shown himself to be not only a faithful pupil of Darwin, but one who on several occasions has been more ultra-Darwinian than perhaps Darwin himself would have been.

Those who consult the very voluminous literature of the subject will soon be convinced that the number of biologists who have preferred patient experimentation to theoretical speculation is very limited indeed. Experimentation on this subject demands a great deal of time, of patience, of devotion, and is liable to meet with many pitfalls. Yet for those who came after Darwin this should have been the task that lay closest to their heart: to test the two great groups of facts on which descent and selection are founded, by means of new and more detailed experiments.

These two groups of facts are the phenomena of heredity and of

* This article was written in English by Professor Hubrecht, the eminent Dutch zoologist, who has an equal command of the French and German languages. Professor de Vries is at present in the United States in order to lecture at the University of California and other institutions.—Editor.

variability. Heredity, the conservative factor, by which what was once acquired is multiplied and rendered stable; variability, by which there appear side by side with those forms that are hereditarily constant, others which, being perhaps in yet closer harmony with the environment and with the prevailing conditions of life, may thus obtain a chance to defeat the first and to supplant them, with the prospect, however, of being in turn ousted by yet more closely adapted new forms, more exactly fitted to the surroundings. The mutual interaction of these two factors, heredity and variability, shows that a continual tendency in organic nature prevails, by which life proceeds from the simple to the more complicated, from the more primitive to the more perfect. And the archives in stone that have been opened to us by the geologists contain ample proofs to convince us that during the succession of thousands and thousands of centuries, plant life and animal life all over the world have passed through a similar process of development.

When Darwin was writing his 'Origin of Species' the chapter on 'Heredity' in physiology was yet a book sealed with seven seals. During the last forty years, especially during the latter twenty, several most important pages of that book have been closely studied and partly deciphered. The conviction has begun to dawn upon us that the phenomena of heredity, assimilation and growth do not belong to different categories, and that, furthermore, this so mysterious heredity can be traced to the minute material particles with which it is bound up, and which through the whole range of plants and animals show an unexpected uniformity.

The labors of such men as Hertwig, Boveri, van Beneden, Strasburger, Guignard and many others, who have made a study of heredity, are especially important, because they have succeeded in analyzing the phenomena into their component elements, thanks to careful observation and experimental testing. They have shown us how very much we may yet expect from experimental work. In the next few years we may, no doubt, look forward to a rich harvest in this extensive field of investigation.

Variability is the name of the second group of facts, on which the slow development of higher, better and more complicated types of living beings rests. Here, too, we must call for the facts and ask for the credentials of the theories we meet. There is ample proof that in the domain of variability we encounter many delusive traps and a host of difficulties. Even Darwin has not spun out to the end the thread of comparative experimentation. It has been reserved to Hugo de Vries to point this out in a very remarkable book ('Die Mutationstheorie,' Leipzig, 1901–1903) that has just been completed.

For nearly twenty years Professor de Vries has been busy making

experiments on a large scale on variability in plants, and the results of those experiments have enabled him to formulate gradually his own thoughts and to compare them with the teachings of Darwin, Wallace and others. Those who are acquainted with the patient experiments of the Amsterdam professor and have assimilated his results, will agree that, by the publication of this book, a new leaf has been turned in the history of evolution. A set of new facts has been gathered with which all those who of late years have theorized about Darwinism must reckon, and which will undoubtedly prove to be the starting-point for further experiments in the direction they so clearly indicate.

I shall now try to point out: (1) in what respects de Vries's work is such a very decided step in advance; (2) in how far it might be supposed that there is any conflict between Darwin's opinions and those of de Vries; and (3) to what extent a teleological interpretation of nature might draw upon the results of de Vries's investigations.

Supposing there existed no variability in nature, life would lose a good many of its attractions. Fancy men and women resembling each other like so many drops of water, both physically and morally! Fancy all dogs constructed according to one pattern, all flowers, all trees of one species being absolutely identical as to their branches, number and shape of leaves, etc.! Fortunately, from our very childhood we have learnt to see nature in a different light, and we have all contracted the habit of giving our preference to the finest and best horses, flowers and playfellows; permanent selection is thus being exercised by us, which can add much to our happiness in life. In effecting it, we make use of what variability offers, and, consciously or unconsciously, we always tend to favor the better and to decline the worse.

Yet more intensely than in the way just sketched, the variability of living organisms is utilized by those who make their livelihood by the rigorous application of selective principles to plants and animals. Dealers in seeds of improved plants, nursery gardeners who cultivate rare varieties of flowers, breeders of birds and domestic animals, all these have a direct interest in every change for the better or for the worse, and are very keen at increasing the former and eliminating the latter. Any one who sells corn or maize, which, when sown under the same conditions, produces ears doubly full or yielding flour of a better quality, may be certain of a substantial gain. One who cultivates beet-roots containing a greater amount of sugar gains equally. Again, a man who lays out a considerable amount of money in purchasing mares and stallions beautifully fit for racing purposes, and who breeds from these with care, will not only be paid back the money he spent, but find means of quickly doubling his capital.

The improvement of our domestic animals, the development of our wheat, the diversity in color and in shape of our decorative plants,

have long been objects of constant effort. Whole classes are constantly occupied in making the most of the phenomena of variability, as much for their own advantage as for the benefit of the community. If we consider the results thus obtained we can not fail to notice that the plants and animals in question have grown to differ so much from the original stock that, should we meet with them in nature, we should undoubtedly call them new species, perhaps even new genera. It follows that if man can in this way direct the phenomena of variability to his own use, the origin of species of plants and animals in nature may depend on a similar series of phenomena.

In nature, however, the selecting breeder is replaced by an automatic process—the survival of the fittest in the struggle for life. That struggle occurs in the first place between members of the same species; it is a struggle for food, light and air, for fecundation and thus for reproduction.

But such a struggle for existence, by which the fittest remain alive and gradually supplant the less fit, does not take place only between individuals of the same species; it is also waged—and perhaps more effectually—between closely allied species.

In this conception the final decision will be reached by the cooperation of very numerous circumstances. It finally leads to a sifting process, to the disappearance of many and to the selection of a few. Selection is a self-regulating phenomenon; it is nature that chooses, and the name of 'natural selection' is thus amply justified. According to Darwin and Wallace, who simultaneously formulated the principle, the origin of species is brought about by 'natural selection.' It is the counterpart of the voluntary or 'artificial selection,' to which man owes the improvement of various cultivated plants and domestic animals. The material out of which in both cases new races and new species are being created, is that which variability offers: the struggle for existence in nature, the breeder in his hothouses or in his kennels, shapes the material into new races, varieties and species.

We thus find ourselves compelled, whenever we wish to penetrate more deeply into nature's laboratories where new species are being fabricated, to sift most carefully the whole and complicated set of phenomena which we call variability. Only in this way may we hope to approach by means of our imagination the coming and going of different forms of organized life that have succeeded one another since the cooling of our planet, and of which only a small portion have been preserved as fossils since the Silurian epoch.

Darwin inaugurated the sifting process with wonderful sagacity. Wallace has continued the work, but has wandered away from reality (as de Vries will teach us) to a considerable extent. Darwin was well acquainted with the fact that two kinds of variability should be dis-

tinguished: one, which is called fluctuating variability, oscillates round a mean value; we shall consider it a little more closely. Whichever characteristic of a species we happen to choose, we shall always find, in considering a number of specimens, that individual differences, individual variations, can be noticed which, when tabulated according to size or to number, do not exceed two opposite extremes. Half-way between these extremes we find the 'norm' for that particular characteristic. The fluctuation may be represented by a curve, the culminating point of which corresponds to the norms just mentioned, whereas to the right and to the left of it the curve gradually approaches the horizontal line and has a symmetrical shape. Quételet and Galton have insisted on the great significance of the fact, that fluctuating variation remains enclosed within the limits of such a curve of regular shape; the curve itself is, therefore, often called Galton's curve.

Not only for plants, but also for animals and especially for man, the existence of such Galton curves, expressing the amount of variability, has been definitely established by different observers in very numerous instances. Thus, for example, Ammon has obtained his material from South German recruits. We need not insist on the fact that the greater the number of cases, the more reliable the curve.

The different degrees of fluctuating variability can undoubtedly be seized upon by any one who wishes to make them the starting-point for the breeding of certain distinct variations. Thus, for instance, by constantly selecting for the reproductive process those plants in which a given deviation is strongly marked, after a certain time and after a series of generations, a plant can be obtained for which the Galton curve would indicate a displacement of its culminating point in the direction of the selected variation. In this way an increase in the yield of sugar obtained from the beet roots has been arrived at from about 7 per cent. to 13 or 14 per cent. Thus also ears of maize have been produced that bore 20 rows of grain, whereas the kind from which the experiment had started always bore 12 to 14 rows.

As soon, however, as such conscious and voluntary selection ceases, the next generations successively return to the original curve. In order to prevent this retrograde process, without a constant and repeated application of the artificial selective process, we are obliged to prevent the appearance of new generations, by forcing the plant to reproduce itself not by seed, but asexually by means of buds. It is well known that definite kinds of delicate fruit are reproduced in this fashion, because if multiplied by seed, they would always tend to fall back into their former state of less value. Transposing the culmination of the curve of variability artificially, as explained, or breeding variations to the right or to the left of the norm, can never exceed certain limits. Agencies are at work there which prevent the fluc-

tuating variability from going any further. The existence of such limits compels us to acknowledge that there is no possibility that species might arise in nature according to the same plan by which certain breeds originate under artificial selection.

On this point de Vries and Wallace differ essentially. The latter is convinced that the fluctuating variability is the only source from which new species have gradually originated; de Vries, however, is quite justified in claiming that the examples of the increase and the accumulation of certain variable characters do not prove that a new species or subspecies has ever arisen in that way in its natural environment.

But in addition to the fluctuating variability we have now to consider another variability, regarding which both Darwin and Wallace have collected numerous data, the so-called 'single variations,' which do not follow the Galton curve. They are not connected with their starting-point by very gradual transformations, but are separated from it by a measurable distance which they have overcome, not by degrees, but by starts. They have, therefore, been named 'sports' or 'saults,' the leap being in different cases larger or smaller.

We have seen that fluctuating variability leads to slow changes and furnishes farmers with the material to improve the races of animals and plants. The 'chance variations' in their turn are valued quite especially by horticulturists and nursery-gardeners.

The English name 'single variations' expresses very well, indeed, the difference between the two kinds of variability. Fluctuating variability shows us simultaneously all the different degrees between extremes, as represented by the descendants of a single parental pair. The single variations, on the contrary, stand isolated; they are discontinuous; between them and the original parent form we do not observe any gradation. This difference has long been noticed, and on several occasions the difference between these 'single variations' and fluctuating' or 'oscillating variations' has been insisted upon.

De Vries has accepted the name 'mutation' and has submitted the phenomenon to a severe experimental test. The chief result of this has been the conclusion which has at the same time become the basis of his own mutation theory—that by means of fluctuating variability certain local and improved races may indeed be bred, but that in nature new species never arise through its agency. These latter owe their origin exclusively to mutation, to 'discontinuous' variability. He is here entirely opposed to Wallace, who looked upon fluctuating variability as the real source from which species gradually originated.

With Darwin, de Vries is less at variance, and a quotation from the 'Origin of Species' leaves no doubt that Darwin fully appreciated the value of the single variations for the formation of new species. We read on page 66 of the edition of 1872:

> In order that any great amount of modifications should be effected by a species, a variety when once formed must again perhaps after a long interval of time, vary or present individual differences of the same favorable nature as before; and then must again be preserved and so onward step by step.

These lines contain an abstract of de Vries's mutation theory. And then further, on page 72:

> It should not, however, be overlooked that certain rather strongly marked variations, which no one would rank as mere individual differences, frequently occur owing to a similar organization being similarly acted on. . . . There can also be little doubt that the tendency to vary in the same manner has often been so strong that all the individuals of the same species have been similarly modified without the aid of any form of selection. Or only a third, fifth or tenth part of the individuals may have been thus affected, of which fact several instances could be given. For cases of this kind, if the variations were of a beneficial nature, the original form would soon be supplanted by the modified form, through the survival of the fittest.

Again, when Darwin denies having said that time alone plays a part in the process of modification which changes one species into another, he writes (p. 82, *l. c.*):

> Lapse of time is only so far important, and its importance in this respect is great, that it gives a better chance of beneficial variations arising and of their being selected, accumulated and fixed.

In the fragments which I have quoted Darwin appears to have had before his mind mutation, not fluctuating variation. And I must insist on the fact that de Vries makes a point of showing that Darwin was decidedly inclined to accept the process of mutation. De Vries quotes (p. 25) from Darwin's 'Life and Letters' (p. 87, Vol. II.) and from the 'Origin of Species,' *e. g.*, the following words:

> The formation of a . . . species I look at as almost wholly due to the selection of what may be incorrectly called *chance* variations . . . unless such occur, natural selection can do nothing, and he adds: It is obvious that Darwin has attributed a great and often a preponderating, perhaps even an exclusive significance to the single variations. . . .

The chance variations were not for Darwin the extreme cases of fluctuating variability, that can be everywhere observed; they were fortuitous phenomena. For these natural selection is always on the lookout, or as Darwin has it, metaphorically, 'He catches hold of them, whenever and wherever opportunity offers.' Darwin must have been inclined to think that these variations, these mutations, arise in accordance with certain laws which are entirely unknown to us. In consequence of the operation of these laws at least a certain number of favorable modifications must inevitably arise after a given lapse of time. Hence the gradual evolution which most living organisms have undergone in the course of ages.

Darwin also undoubtedly suspected the existence of a certain periodicity. 'Nascent species are more plastic,' he says; and he

thereby intends to imply that they form more numerous single variations and have thus a better opportunity to split up again into further species—so far de Vries (pp. 24–26).

I have purposely insisted on these points, because here and there a tendency seems to prevail to look upon Darwin's views on the origin of species as unsatisfactory and obsolete, and to proclaim the necessity of replacing them by a brand new hypothesis with which the name of de Vries should be coupled. These tendencies are in great favor with those that bear a grudge to the so-called Darwinism for other than scientific reasons, and who in their innermost heart would at the same time like to see a similar fate reserved for de Vries's demonstrations, and even for the whole theory of evolution.

We have, however, seen in de Vries's own words how little he considers himself an antagonist of Darwin. On the contrary, his great and imperishable merit consists in this, that his important and extensive experiments have provided us with a reliable basis concerning a subject about which Darwin had not fully made up his mind.

Darwin seems to have suspended his judgment; at all events, he has not drawn a hard and fast line between the results which artificial selection can attain when applied to fluctuating variations, on the one hand, and to mutations, on the other.

The experiments which de Vries has continued during many years on the two divergent processes, which Darwin has not sufficiently kept separate, have justified him in claiming that now, for the first time—forty years after the appearance of the 'Origin of Species'—the actual birth of a species has been observed by him. He has thus opened up a most extensive field for further investigations by other naturalists, and he has undoubtedly put an end to useless polemics which often threaten to become yet further burdened by subtleties.

Far from having undermined Darwin's Darwinism, de Vries has completed, purified and simplified it. To Wallace's Darwinism, however, de Vries has dealt a severe blow, Wallace having attached no significance to 'single variations' as possible sources of new species; whereas Darwin has always continued to acknowledge their importance as such, even though he did not thoroughly understand the laws to which fluctuating variability is subject. Even Weismann, who has only partially appreciated Darwin's philosophic indecision and who has, without wavering, followed a road which has now landed him in his 'germinal selection,' has undoubtedly taken notice of de Vries's experimental treatment of the subject with interest, though probably not with personal satisfaction.

Let us now try to picture to ourselves what conclusions de Vries has been able to reach experimentally with respect to the phenomena of mutation, and what he has taught us concerning the all-important question: How have species originated?

De Vries has started from the phenomenon above mentioned, so well known to nursery-gardeners and which is often of considerable financial importance to them; the phenomenon that suddenly in some of their flower-beds single variations appear which are constant when reproduced by seed. By systematically propagating these exceptional specimens a quantity of seed may be obtained in a few years that can be brought into the market. Soon, however, the variety loses its mercantile value; seed may now be obtained by anybody in increased quantities from the plants that have already been sold.

Are there any of our native species of plants in which the same phenomenon produces itself naturally? was the question which de Vries set himself. And if so, can they teach anything about the formation of species? In commencing the inquiry he started with about a hundred different species. Of all these, only one exhibited the property sought for—this, however, in such a way as to throw full light on the subject in most unexpected directions.

The species of plant which de Vries actually managed to detect in the act of 'mutation' on certain fields in Graveland, and which has continued to do so with perfect distinctness during many years in the Amsterdam Botanical Garden, bears the name of *Œnothera Lamarckiana.* It is one of three species of the genus that have been brought over from the United States and is now running wild in Europe.

De Vries has thus convinced himself that the great majority of plants about us do not show cases of 'chance variations,' 'mutation variation' *per saltum;* in other words, that the species that have been observed for many centuries may be said to be stable, invariable. But they are stable in so far only as—perhaps with very long pauses—periods of mutability appear, during which, next to the stable central species, new sub-species appear that are also stable when propagated by seed. Further experiments, however, are required to throw light on the periodicity.

Another conclusion was this, that the species which do produce *mutations* bring forth not a single mutation, but quite a number of them, varying among themselves. The mutations occur both in plants growing in the wild state and in those samples of *Œnothera Lamarckiana* that are bred under supervision. Their frequency, as determined by exact statistical tables drawn up by de Vries, varies between one and two per cent. Of the 50,000 *Œnothera* which de Vries has observed during ten years' culture, there were 800 that could not be designated by the name *Œnothera Lamarckiana.*

A somewhat skeptically disposed person may claim that this number of 800 indicates the number of the most marked deviations which were noticed among 50,000 plants; that, in other words, they are individual, fluctuating variations that would also be found in the same

quantity among 50,000 other plants. To this it may be replied that the phenomenon of fluctuating variation, as it appears in *Œnothera*, has been studied in detail by de Vries and has been exactly determined both for the central species and for the different subspecies (mutations). In all of them it occurs on a large scale, but not one of the specimens above mentioned belongs to it. These 800 have very special characteristics, by which they can be sharply distinguished from the fluctuating variations. And, as is especially remarkable, they are not in every respect different from each other, but may be arranged in seven natural groups, each of which possesses exactly the same systematic value as that particular combination of specific characteristics to which the name of *Œnothera Lamarckiana* has been applied.

The number of individuals of those seven groups, of which de Vries has observed the spontaneous appearance, is, however, most unequal and varies between 1 (*Œnothera gigas*), 56 (*Œnothera albida*), 350 (*Œnothera oblonga*), 32 (*Œnothera rubrinervis*), 150 (*Œnothera nanella*), 221 (*Œnothera lata*) and 8 (*Œnothera scintillans*).

De Vries has studied the mutations thus arising, some of which are rare and some more common, with the utmost care, and has followed them during the whole of their existence.

*Œnothera gigas*, which has only once arisen in the Lamarckiana group and which is characterized by much larger-sized flowers, a different shape of the leaves that form a rosette at the root, more thickly set leaves along the stem, etc., was sown by de Vries in 1897, after he had obtained seed, thanks to artificial fecundation, the possibility of self-fertilization being excluded. He thus obtained 450 plants, which, with the exception of a single one, exhibit all the characteristics of the *Œnothera gigas* with perfect constancy. The exception was not a retrogression towards *O. Lamarckiana*, but a new deviation, provisionally indicated as *O. gigas nanella*. From 1898 to 1900 further propagation by seed has been effected during three generations and under very strict precautions; and until now all the descendants of that one mutation of 1895 remain perfectly constant; de Vries has actually *seen* the species *O. gigas* come forth out of *O. Lamarckiana*, first in nature, afterwards in his own nursery-garden. It appears to be a very strong plant, which, if it had to fight for its existence against *O. Lamarckiana*, in equal numbers and under the same circumstances, would probably prove to be the winner.

The second mutation, *Œnothera albida*, which occurred 56 times during the experiments, shows another character. It is a feeble plant, and was originally considered a pathological deviation, which, however, in the later generations has proved itself to be none the less constant, and, though but little fertile, produced 86 plants in 1898 and 36 in 1899.

The third mutation, called *Œnothera oblonga,* repeatedly occurred (350 specimens) in the series of generations of *Œnothera Lamarckiana* that were successively cultivated. Many hundreds were cultivated later. It may be recognized with certainty as soon as the sixth leaflet unfolds, and has remained unchanged, with the exception of two specimens, which, however, have not retroceded towards *O. Lamarckiana,* but showed the characteristics of *O. albida* and *O. rubrinervis.* This mutation has thus, although perfectly stable, retained the power to further mutate.

The fourth mutation, *Œnothera rubrinervis,* again shows other interesting peculiarities. It is a strong plant, not less rich, both in pollen and in seed,* than *O. Lamarckiana,* which was more or less the case with the other mutations. It has been obtained in very great numbers (2,976 specimens) by de Vries, and the stability of *Œnothera rubrinervis* has asserted itself most distinctly also for all those specimens that had descended from different mother plants.

The fifth mutation, *O. nanella,* differs from the others in the fact that its deviation from the original *O. Lamarckiana* does not show itself in a given number of sharply determined and constant characteristics, but only in one, its dwarfed dimensions. Thus we should be inclined to look upon *O. nanella* rather as a variety than as an elementary species. However, in this case the smaller dimensions do not come under the head of fluctuating variability, but we have undoubtedly an unmistakable mutation that can be recognized as such with certainty as soon as the second leaf begins to show itself, and which, when fecundated in 1893 by its own pollen, directly produced 440, and in 1895 2,463 germinating plants, all of them, without exception, *Œnothera nanella.* In 1896 the seeds of 36 other plants of *O. nanella* were again planted and 18,000 seedlings were obtained, which again showed, with perfect precision, the characteristics of the species, with the exception of three mutating plants that bore at the same time the distinctive characters of *O. oblonga,* and thus formed an elementary species of the second degree, *O. nanella oblonga.*

We have yet to mention two mutations, *O. lata* and *O. scintillans.* The first only consists of female plants; there is never any fertile pollen produced, so that its stability can not be made out with certainty. The second, a dark green plant with shining leaves, is a rare mutation,

* It should here be mentioned that de Vries has noticed (*l. c.,* p. 186) that the seeds of mutating plants generally retain the power of germinating for a longer period than the seeds of the normal *O. Lamarckiana.* Upon this fact he bases the expectation that perhaps later it will be possible to utilize this peculiarity and to find means to increase the percentage of mutating plants in a given series of sowing experiments by artificially somewhat accelerating the dying off of the seeds. This might also prove important when searching for mutations.

which especially differs from those we have been studying, by the fact that even when artificially fertilized, with the utmost precautions, the mutation is not stable. The majority of its descendants belong to three groups: *O. scintillans, O. oblonga, O. Lamarckiana.* As compared with the wonderful stability which we encountered in the preceding mutations, the lability of this one—which at the same time seems to follow a certain law—is a most remarkable phenomenon, the origin and the significance of which have still to be traced.

We have now seen that for a number of years de Vries has been able, by his careful and skilful experiments, actually to witness the very process of the origin of species in nature. On the particular spot near Graveland, where he first noticed the process of mutation in nature, it had, of course, been going on even before his first observations. He here encountered, besides the *O. Lamarckiana,* a second species, the *O. lævifolia,* with which he also made experiments in the Amsterdam hortus, and which also produced numerous mutations, some of them identical with those obtained from *O. Lamarckiana.*

Especially important was the irrefutable demonstration of the fact that the process of mutation does not appear as one single specimen which gives rise to a constant variety, but that it again and again repeats itself in every generation in a certain percentage, and that absolutely the same 'mutants' appear—even though in varying quantities. The individual mutants thus find their chance of surviving considerably increased and, as soon as a slight change in the outward circumstances occurs, any mutation, which at the outset was in the minority as compared with the parent species, may slowly but surely become the majority. It is constantly getting a fresh supply from the parent species and, as soon as it shows itself better adapted to the circumstances, it may finally supplant the parent species entirely.

This struggle for existence does not occur between individuals of the same species, but between the mutations and the parent species. As long as the mutation has not appeared, there can be no question of the origin of a new species; the species is then constant, and only submitted to fluctuating variability, which can produce local races (not elementary species) under the constant cooperation (either artificial or natural) of selection, but which never leads to the formation of species. During a period of mutation a modification of the species is not always a necessary consequence; for in many cases the parent species will prove to be the fittest, and the mutations will then none of them be permanent.

The fact has been established by de Vries that in the natural life of a wild plant a series of phenomena occurs which justifies us in saying that he has made us see and actually touch the origin of species, whereas Darwin had made us understand it.

De Vries has not deviated from the teachings of the master, but he has developed them; he has brought us a further and a most important step forward and he has paved the way for later investigators. Theirs will be the task to make out in how far the laws of the mutation process, which de Vries has for the present only been able to make out for one genus of plants, also apply to the other plants and to animals. These laws are: (1) New elementary species arise suddenly, without transitions. (2) New elementary species are generally perfectly stable from the very first. (3) Most of the new types have all the qualities of elementary species, not of varieties.* (4) The elementary species usually appear in a considerable number of individuals simultaneously, or at least within the same period. (5) No important relation whatever exists between individual variability and the new qualities of the elementary species. (6) The mutations, which give rise to new elementary species, take place in the most various and divergent directions. The modifications concern all the organs and are of the most varied descriptions. Part of the new types perish without descendants. Among the others, natural selection must slowly decide. (7) The phenomenon of mutability appears periodically.

Some of these laws of the mutation theory require further explanation. In the first, the *sudden* appearance of new elementary species is formulated. The characteristic qualities of the species thus arise *per saltum,* without transitions, such as are always observed in fluctuating variations. The ancestral forms of the different *Œnothera* 'mutantes' were perfectly well known. It is a fact that every mutant has been obtained from seed of normal and carefully examined *O. Lamarckiana.* On every occasion the new mutation suddenly appeared in all its details. The name 'elementary species' is given to the new form, and here we enter the domain of terminology and must necessarily furnish some explanation.

What is a species, what is a new species? What is an elementary species, which has also been called a subspecies? Are these last different from races and varieties? If so, how? For those who are not naturalists all these questions seem to be frivolous. They know that Darwin has written a celebrated work on the 'Origin of Species' and that a century earlier Linnæus had instituted for species in nature the 'binary nomenclature,' so that *'Bellis perennis'* stands for daisy, *Elephas indicus* for the Asiatic, and *Elephas africanus* for the African elephant. And so, according to their lights, zoologists and botanists will, by this time, have agreed on what a species is. This, alas, is far from being the case! The Mosaic belief in the separate and inde-

* "Many of my readers," says de Vries, "will be inclined to call my new species varieties, *just because I was able to trace their origin.* This is a mere verbal contention, of no importance at all for science."

pendent creation of every species at least furnished us with a distinct definition, even though transcendental. But as the idea of a slow and gradual evolution in nature has come to predominate in the course of time, sharp boundary lines have been effaced, and species have become both of an artificial and of a temporary nature. They have become a compartment into which man temporarily brings together a larger or smaller number of individuals, knowing that in times gone by the contents were confluent with those of another such compartment and that in the far future there will be other changes.

And when in later years the ideas of Wallace, which we mentioned above, found increasing sympathy, the lines of separation grew dimmer yet, and the idea of species became exclusively an artificial limit comparable in musical terms to so many thin lines that mark the bars in the continuous symphony of the evolution of life upon earth.

Thanks to de Vries's experiments, which have enabled him to formulate his mutation theory, he has now provided us with the means to define species more strictly. The species is limited by space and time. By time, for it begins whenever, by a process of mutation, its peculiar combination of specific characters springs into existence, even though this mutation, unobserved by the untrained eye, can as yet only be detected by the specialist. Whenever mutation appears, the combination of specific characters is modified in the mutants, and at the same time a new species has appeared side by side with the mother species, which itself remains stable.

The distribution of a species in space can be very varied; some are known only from a very limited area, others may be cosmopolitan. The species thus limited in time and space is what de Vries calls an elementary species. It is with these elementary species that the next generation of naturalists will have to grapple when they wish to elucidate evolutionary problems experimentally. The existence of such elementary species is no novelty which de Vries has been the first to make us acquainted with. Linnæus knew these elementary species perfectly well, but he called them varieties and forbade his pupils to waste their time on them. 'Varietates levissimas non curat botanicus.' From his point of view this was perfectly justified. He came forward to restore order in the chaos of classification, and as such he strove to combine the material then available into not too small bundles. His species were what the Germans have called by an expressive name 'Sammelarten,' receptacles, into which the so-called 'varietates minores' were thrown together. According to his idea, the species had been created in the beginning as an entity, the 'varietates' had gradually arisen from it, even though he could not prove this experimentally. With regard to them, Linnæus was an evolutionist, just as, among his predecessors, the idea had long predominated that the genera had been

created, whereas the species had come from these, as so many local deviations.

Who would deny that Linnæus's work has facilitated the task of those that have come after him? Nevertheless, many species have been repeatedly subdivided. We must henceforth admit that when a species goes through a period of mutation, or has just gone through it, the number of elementary species that keep up their independent existence by the side of the parent species may be considerable, as we have seen with *Œnothera*. And it is easily understood that a tendency arises to denominate for convenience those numerous elementary species not by their own names, but by a collective name. This happens in most handbooks of systematic botany for well-known European plants, as, *e. g., Draba verna,* of which not less than 200 perfectly stable elementary species are known, *Viola tricolor,* etc.

Henceforth, however, we may no longer allow ourselves to be guided by opportunism. Systematic botany will have to take her watchword from physiology. De Vries has combined the qualities of the experimenter, who dares to look the physiological problem in the face, with those of the systematist, who observes and appreciates with uncommon sagacity the slightest shades of difference, and who with utmost delicacy of touch sifts and deals with species and races, mutations and variations.

The elementary species are stable. Selection calls forth different races within the limits of these species, but whenever selection ceases the races turn back to the parent form. The maximum deviation in these races is generally obtained after three or four generations of continuous selection; it takes about as many generations to bring back the parent form.

It is superfluous to say that many of these phenomena must be yet submitted to experimental investigation. De Vries has started this, and both in the domain of fluctuating variation and formation of races and in that of crossing and hybridizing he has already partly completed, partly only just commenced, elaborate experiments. Others besides himself have of late years analyzed the phenomenon of variety closely. The Cambridge zoologist, Bateson, has attempted to trace in his well-known work, 'Materials for the Study of Variation,' *what* it really is that variability offers towards the making of species, both in the most different species of animals and with respect to their divergent organs. He has, however, not seen his way out of the labyrinth, and although he came to the conclusion that it is not fluctuating variability which presides over the formation of species, but that a *discontinuity* must necessarily play a part, yet he, too, has committed himself to the assumption that the determination of the width of the fluctuations can furnish us with valuable data for understanding the gradual forma-

tion of species. He, too has not yet succeeded in analyzing and distinguishing from each other—what has been de Vries's merit—variability within the boundaries of constant species and mutability which does not fluctuate, but which by a sudden bound leads to the new species.

Much closer to this valuable discovery we find two students of the fossil-animal kingdom, two paleontologists, one of whom (Waagen), as long as twenty-five years ago, understood the importance of the phenomenon of mutation, even without the support which the series of de Vries's experiments would, of course, have afforded him, while the second (W. B. Scott, of Princeton) has most clearly expressed himself (*American Journal of Science,* 1894) that the formation of species by selection of fluctuating mutations, such as Wallace maintains, is rendered most improbable by what the fossil-animal world teaches us.

This world of fossil animals exhibits in certain regions of the earth, where the successive geological formations have been retained in undisturbed order, a similarly undisturbed ascending series. Far from finding in that series the divergent fluctuations which Bateson had accepted for so many animals, Scott has shown (and has strengthened his argument by referring to the results of many other paleontologists) that these fluctuations are indeed—though exceptionally—found among fossil animals as so many individual deviations (thus proving that also in that time the fluctuating variability existed within the limits of the species)—but he is at the same time convinced that this phenomenon has nothing to do with the slow modification of species, which takes a straight line and not a zigzag one.

Scott, although he was not at that time acquainted with de Vries's experimental evidence, staunchly holds to the idea that species have not grown out of the gradual selection of deviating individuals, but have appeared by mutation, by very small but sudden starts from one stage to the next.

We see before our eyes how the species of the deeper layers are gradually modified as we reach the higher layers; we find that all individuals simultaneously underwent this modification; in other terms, the phenomenon can hardly be described otherwise than by saying that the older species tends directly towards an aim, which the younger species that has descended from it has attained.

Many paleontologists even go so far as to admit a previously determined direction in gradual evolution. There is, of course, close affinity between such a predetermined direction in evolution and the teleological idea of design presiding at the creation of species. Clerical opponents of evolution may here have their chance of adapting the newest results in the study of that process to their personal principles.

Still, although the mutation experiments of de Vries have considerably strengthened the argument of Scott and other paleontologists, that slow, simultaneous mutation has also taken place among those fossil animals—the same experiments have, moreover, proved beyond any doubt that there is no such thing in nature as predetermined mutation in one special direction, but that, on the contrary, mutation occurs in very different and very divergent directions.

When once the mutation process leading to the formation of species has begun, the most different mutations, as we have seen above, arise. From our point of view, some of these may be called good, others bad or indifferent. About the permanence of any of them, it is, however, the surrounding conditions, acting by means of selection, that decide. And often the decision lies in another direction than would have been surmised from the human adjectives just named.

By the phenomenon of mutation the possibility exists that useless, and even to a certain extent prejudicial or noxious, specific characters may appear, a phenomenon which could never be reconciled with the views of Wallace.

For the greater part these characters are sure to be eliminated, but if other circumstances happen to be or to become favorable to a mutation, which was originally without any particular significance, it can then gradually develop and become adapted to certain modifications in the surrounding factors of life. The majority of the mutations, however, soon perish in the struggle for existence. Of those many elementary species that were doomed from the first, nothing has, of course, come down to us in the archives of the fossil remains; only when their number has considerably increased in comparison with the parent species will it have been possible for them to survive, but then they have already risen to be a side branch, or may even be supplanting the parent stock.

The theory of mutation, as well as that which ascribes the origin of species to the selection of fluctuating varieties, enables us to understand how efficiency and adaptation in organic nature have come about by the mutual interaction of natural processes without the aid of supernatural intervention. The struggle for existence between species and mutations comes about in the same way as does the struggle for existence between individuals in the older view. Spencer's expression, however, 'the survival of the fittest,' must henceforth be interpreted as meaning 'the survival of the fittest species.' When we agree with de Vries that the gradual mutation of species is not necessarily the revelation of a foreordained design, this should be interpreted in the spirit of greater humility which befits the naturalist when he is confronted by the gigantic problems of organic nature. As long as a natural coordination of facts furnishes us with an intelligible causal

connection, he does not feel justified in agreeing with those who are ready to accept explanations outside the pale of science. The same naturalist, however, will always be found ready to admit that he is yet exceedingly far from being able to give an 'explanation' of the inner meaning of the real significance of the mutation process.

In order to penetrate into this it is necessary to analyze further the phenomenon of heredity. Concerning this, de Vries has already on a previous occasion published theoretical views which follow in the footsteps of Darwin's celebrated theory of pangenesis. As the chemist operates with molecules and atoms, for the reconstruction of the processes of inorganic nature, so the biologist, when trying to represent to himself living matter, has to take into account the smallest entities, which have received various names from various naturalists, and to which de Vries gives that of 'pangens.'*

Pangens are something different from complicated molecules; they can assimilate and they can reproduce themselves. Not only does all living matter, wherever found, consist of them, but those smallest living particles must at the same time be considered, either individually or grouped together, as being bearers of single or of mutually correlated properties of living matter.

An augmentation or a diminution of the number of pangens which represent a certain property will call forth the phenomenon which we have named fluctuating variations; while a modification in the composition of the pangen, for example, by division in two unequal parts, or by substitution—using a term well known in chemistry—will be equivalent to a mutation (progressive mutation), as will also the disappearance of a determined pangen (regressive mutation). Thus, according to these abstract representations which we form of the mysteries of heredity, the fluctuating variation depends on quite a different category of phenomena from those of the chance variation. And we understand directly that the chance variation obeys a more complicated mechanism than fluctuating variation, which depends only on the greater or lesser numerical importance of the preexisting elements, while the chance variation, the formation of species, implies a change of the existent elements.

As to how this change in the pangens periodically takes place, both simultaneously and successively, among a certain number of individuals, or might be aroused or caused; as to how the unequal division or substitution obeys fixed laws in such a way that the mutants, arranged in groups, are alike—all this for the present can not be explained by us.

If we aim to understand the conditions we shall be able to create species, as we can now breed improved races. And as we gradually

* H. de Vries, 'Intracelluläre Pangenesis,' Jena, 1880.

learn to analyze the elements of the phenomenon, the probability grows that sometime we shall master the art of actually directing the series of natural phenomena. A new and limitless field of work would then be opened. Provisionally we can guess only from what we have as yet observed that certain processes are able to call forth or to accelerate the phenomena of mutation. Thus de Vries suggests the idea that a rapid succession of periods of reproduction might facilitate the reappearance of a period of mutation, whilst others think that transportation into quite different surroundings or transplantation might produce it. Others, again, appear to believe that increased nutrition, either combined with the conditions mentioned or not, would call forth mutation. All this, however, is no more than guesswork and hypothesis. We have as yet no means of fully knowing and of understanding.

For the present it is safer to recognize our absolute ignorance, and at the same time to define more exactly how far de Vries has brought us, and what is the important step for which we have to thank him. We can warmly recommend the reading and studying of de Vries's clearly written and beautiful book. He has been the first to show us the sharp distinction that exists between chance variation and fluctuating variation, and to prove that it is not the latter, but the former, that calls forth in nature the origin of species. He has not yet been able to tell us whether, and, if so, how, chance variation could be called forth artificially by man. The fact that artificial selection of fluctuating varieties, as well as hybridizing, etc., has already led to such indisputable improvements in the different races of animals and plants may, however, give us hope that a conscientious experimenter and close observer, such as de Vries, has still a full store of important pioneer's work before him and may yet succeed in finding how to direct the mutation process. Thus, the origin of species would not only have been studied more closely, but would be subjugated to the human will. After having seen species originate in nature, man would then be able to call them forth. Then only the 'Origin of Species,' to which Darwin has given us such a marvelous introduction, would be revealed in all its details.

# THE IMMIGRANT, PAST AND PRESENT.

By Dr. ALLAN McLAUGHLIN,
U. S. PUBLIC HEALTH AND MARINE HOSPITAL SERVICE.

AFTER the Peace of Paris in 1783, and the birth of a new nation on the American continent, home-seekers arriving at ports of the United States were called immigrants. Previous to the revolutionary war they were known as colonists. The distinction is one of political allegiance. The colonist was an immigrant who desired to make a home in the new country, but to retain his allegiance to his native land. On the other hand, the immigrant, in a majority of instances, expected and desired to change his political allegiance. Even at the present time the government of Italy regards as Italian colonists all Italians in America who have not been naturalized. If we except the question of political allegiance there was little difference between the colonist and the early immigrant. There were no large centers of population such as exist to-day to invite the parasitic class, nor were there large factories, mines or mills, to demand a supply of unskilled laborers. The country, except a narrow strip along the Atlantic seaboard, stretched in an almost unbroken wilderness far to the west. The type of immigrant willing and able to brave the dangers and hardships of the new country, and hew out a home in the heart of the forest, was necessarily brave of heart, and strong of hand, the very best type of an immigrant—the pioneer. The immigrant of those days was not allured by the promise of high wages, nor by the desire to better his financial condition, but was actuated chiefly by the desire to create a home, and free himself from the trammels and persecutions of the old world. He was at once a pioneer, a woodsman and a farmer. He left behind him many evils, coercion, compulsory military service, religious or racial persecution, grinding taxation, wars in which he had no interest, and prohibitive systems of land tenure. He found in this country, land for all, absolute freedom from racial or religious persecution, personal liberty, and respect for the rights of the individual, regardless of social position. The many advantages offered to the home-seeker who was brave, willing and strong, in the new United States, attracted many thousand immigrants, and it is estimated that one hundred and fifty thousand settled in the country between 1783 and 1810. These early immigrants were mostly from the British Isles, with a few Germans, French and Scandinavians.

The strained relations with England followed by the war of 1812, practically stopped immigration for several years. During 1817, however, twenty thousand immigrants arrived in the United States. This number was unprecedented at that time, and caused considerable criticism of the overcrowding of immigrant ships.

Immigration first assumed large proportions during the decade 1831–1840. It increased progressively, and during the next twenty years was relatively greater in proportion to the native population than at any other period. The great famine in Ireland greatly increased Irish immigration. German immigration was increased at the same time because of industrial depression and the revolt of 1848. The discovery of gold in California, no doubt, also contributed to the increase of immigration at this time.

Irish immigration reached its height in the decade 1841–1850, when it constituted 46 per cent. of the total. It has declined steadily and is now only 4 per cent. of our total.

The Germans kept coming in increasing numbers and in the early eighties were 30 per cent. of the total. They also have fallen off, and now constitute less than 10 per cent. The Scandinavians became a considerable factor in the decade 1861–1870, and in 1889 furnished 10 per cent. of our immigrants. Their proportion has also declined and at present is about 10 per cent. With the decline in the proportion of immigrants from the United Kingdom, Germany and the Scandinavian countries, a rapid increase in the arrivals from Italy, Austria-Hungary and Russia is noticeable.

This marked change in the complexion of immigration can be appreciated from the fact that in 1875 we received 3,631 from Italy, 7,658 from Austria-Hungary and 8,981 from Russia, while in 1903 we received 230,622 from Italy, 206,001 from Austria-Hungary and 136,093 from Russia. In other words, the immigration from these countries to 1875 was only 9 per cent., while to-day it constitutes about 67 per cent. of our total immigration.

In general, the immigrant of the past differed greatly from the immigrant of to-day. As has been stated, the first immigrants were pioneers and differed little from the old colonists of pre-revolutionary times. As time went on they spread from the Alleghenies to the Mississippi, side by side with pioneers from the New England and Southern states. These immigrants were agricultural in occupation and were invariably home-seekers.

The development of our vast natural resources, particularly coal and iron, created a demand for a new type of immigrant, an unskilled laborer, who may be styled an industrial immigrant. The building of the great transcontinental and other lines of railroads furnished additional work for this industrial immigrant, and opened up vast new

fields hitherto inaccessible to agricultural home-seekers. Of late years, most of the desirable, arable land, profitable and fertile without irrigation, has been taken up, and the advantages offered the agricultural type of immigrant in the west have been materially lessened, but our wonderful industrial growth still demands and attracts the strong willing unskilled laborer, and this demand will probably last for many years to come.

The development of our great manufacturing industries also attracted great numbers of skilled artisans and mechanics. At first these skilled laborers were necessary. The necessity for their coming has now disappeared, and not only are they unnecessary for development or progress along industrial lines, but they enter into direct competition with American mechanics and artisans. These may be classed as competitive immigrants.

The rapid growth of our large cities, the establishment of great centers of population, most marked in the past twenty-five years, attracted another class of immigrants, who can only live in such environment, who are simply human parasites unable to exist by their own effort.

Thus immigrants of to-day can be grouped under four heads, (1) agricultural, (2) industrial, (3) competitive, (4) parasitic. The agricultural class includes farm laborers and those desiring to take up land for settlement. The industrial class includes the great army of unskilled laborers, who seek employment in the mines, mills, great works of construction and manufacturing concerns. These two classes are valuable and necessary for the development and industrial progress of the country. The competitive class takes in the skilled laborers, mechanics, artisans and others who come here and enter into competition, in their respective callings, with Americans. This class is not necessary for our advancement and may or may not be of value to the country. The fourth or parasitic class is, as its name implies, not only valueless, but decidedly detrimental to the body politic. In this class are included the peddlers, fakirs, paupers, etc., who congregate and will live only in the large centers of population and who can not or will not do hard physical labor. This class constitutes a load to be carried, and their deleterious influence on the vigor of the nation is in direct proportion to their numbers.

Social and political conditions in Europe determine to a large extent both the quantity and the quality of our immigration. A country well and justly governed and which is in a prosperous condition is not likely to send us many good immigrants. The type of Englishman who would be welcome here as an immigrant, the sturdy Anglo-Saxon yeoman, of whom we delight to form a mental picture, finds conditions of life so suited to him in England that we rarely see him as an

immigrant, and we are much more likely to receive as our English immigrant the degenerate product of the East London slums. The same has been true of Germany for many years, the prosperity of the country, the growth of national pride and reconciliation to the form of government have cut down the German emigration from the great exodus of the eighties to the comparatively insignificant figures of to-day; and the German immigrants to-day do not compare favorably with their countrymen who came here twenty-five years ago. It will be seen, therefore, that it is unwise to consider an immigrant good because he is of one race, or worthless because he is of another. They must be measured individually irrespective of race or creed, for it is better to receive the robust pastoral or agricultural immigrants from countries where the intellectual status, perhaps, is not high and the school system faulty, than to receive from countries, possessing a high intellectual status and a superior educational system, the urban degenerate, criminal, diseased and defective.

To-day we receive the agricultural home-seeker as in the early days of this country. We demand and receive the industrial immigrant, the unskilled laborer who was unknown as a type fifty years ago. And we also receive against our own will the human parasite who remains and can only exist in the great centers of population.

The work which will be done in the next twenty years to reclaim the arid land by irrigation will be genuine empire building and provide thousands of homes for agricultural settlers. No doubt proper care will be exercised by the government to prevent this reclaimed land from falling into the hands of speculators, and the bulk of it will be available for the immigrant of the future.

# WHY IS THE HUMAN EAR IMMOBILE?

By Dr. WALTER SMITH,
LAKE FOREST COLLEGE.

THE ear has had a varied history. The evolutionist has a remarkable story to tell when he recounts the steps in the making of this organ. He traces the opening of the ear to the gill-slits of the fish forms of whose lineage we are. He shows (though this subject concerns us less at present, and is still discussed with some uncertainty as to details) how various structures in the region of this opening, which had originally a different purpose, were modified to become the series of little bones that propagate the vibrations of the air from the tympanic membrane to the fluid of the inner ear. He shows further, with greater or less completeness, how the cartilage shell grew on the outer side of the head, and was supplied with muscles, so that it could be moved about and even have its shape changed.

To get a good illustration of the mobile ear we only need to watch such an animal as the horse; the ear is as mobile as the eye, or more so. The poet speaks of the horse's ear and eye as twinned; but it is interesting to notice that each of the horse's ears can work independently. And it is evident that nature at one time meant, so to speak, that man's progenitors should possess ears of similar mobility. She gave them the projecting frame of cartilage and she attached to it the muscles for its movement. Then in the course of the generations she changed her mind and withdrew what she had bestowed. The cartilage shell, curiously wrought, is still there, and we regard it as adding to the beauty of the head; yet it is probably only a rudiment. The tip of the ear, when present, is a small outgrowth on the outer fold of the cartilage and is turned towards the center of the ear. The muscles of the ear, seven in number, are also rudimentary. Occasionally an individual is found who can move his ears; but even these movements are generally of an abortive kind, and are so unusual that the sight of them may distress those who are sensitive.

Why has man lost this power? Is it simply a case of retrogression? Or is it a loss for the sake of a greater gain, possible only through it? I think the reasons for this change in the organism can be indicated; one can, at least, point with assurance to a great mental gain in which it has resulted.

It will prove helpful to an appreciation of this gain to inquire first what man has lost in the passing of this mobility. The signifi-

cance of this loss can be better understood if we consider for a little such a sense as sight.

The eye is movable and is continually shifting its position. From this mobility two results follow which in the present connection it is important to notice. First, the eye can be readily turned so as to get the clearest vision of any object that is to be examined. We do not see equally well with all parts of the eye; we see most distinctly with the central part directly opposite the pupil, and when there is anything seen out of the corner of the eye which we wish to observe more closely, the eye is, in normal circumstances, turned upon it so as to catch the image in this central part. Spontaneously and accurately these changes in the eye's direction are made. It can readily be understood that great help to our perception is gained from them.

There is another result of this free movement which is of equal importance. To it is due the orderly spatial arrangement presented by the world of our vision. It may seem that our knowledge of the position of things in relation to each other is natural or instinctive, and we may be pointed to the behavior of many animals which are able to guide their movements correctly as soon as they enter the world. But such reflex activities do not seem to be strictly parallel to those of the human child. That the child has in its nervous system inherited a predisposition to its future adjustments, may be true. But it is also true that it does not respond to its surroundings as the chick, for instance, does; it gains its conscious appreciation of external relations by experience. What the child's first experience of sight is, it is difficult for the adult to guess; yet some of our perceptions approach to it. The German psychologist Volkmann von Volkmar calls attention to the fact that when we gaze into the blue depths of the sky our color perception has a character similar to that of a musical note. Probably our visual sensations are all, in their original intrinsic nature, of this sort; color feelings with no idea of position as yet developed. It is further to be noted that we might have a succession of color pictures, such as can be afforded by familiar mechanical devices, without any suggestion as to the spatial relations of these various pictures. And were the eye incapable of movement of any kind, its experiences would be a mere succession of vaguely voluminous color-feelings. But, on the other hand, let the eye be considered as capable of movement, and as free to play among these colors; it passes, say, from the image of the door to that of the wall, and then to that of the window. It is not less important to notice that it can by its power of movement reverse the series and pass from window to door. Such series may, to an indefinite extent, be increased, repeated, reversed. Thus the mind gets the idea of a series of images relatively permanent, always open to observation, and arranged in a perfectly

definite order. It is thus by virtue of the eye's movements that there is secured the perception of the orderly spatial arrangement of the world revealed to our vision.

Further evidence of the important functions which the eye's movements fulfill in perception might be adduced, but it is not desirable for the purpose in hand to take up what is more complicated and debatable. What has been said may suffice to call attention to the great significance of the mobility of the sense organ.

Evidence not less striking might be brought forward in regard to the sense of touch; it might be shown that it is by the movement of the sense organ, say, the finger tips, that explorations of the body under investigation can be, in ordinary cases, best accomplished, and that it is by the producing and reversing of series of touch sensations that the spatial relations of tangible objects are clearly recognized.

The ear is immobile. Accordingly it is incapable of reflex movements for catching sounds, like those by which the eye is turned so quickly to meet the light coming from an object.

We find likewise that the perception of space by means of sound is in an extremely undeveloped form. Many have gone so far as to deny that sound has any spatial character. Yet surely this view can not be maintained. We locate sounds to the right or left, behind or in front; moreover, we distinguish sounds as differing in volume.

Yet it can readily be seen that the spatial characters of sound do not compare in precision and definiteness with those of the sensations of color and touch. What is the size of the thunder? The question at first seems absurd. Yet it can not be entirely absurd, for we speak of the peal as heaven-filling. The appearance of absurdity is due to the hopeless vagueness of the sound image in respect to extent.

If we analyze this vagueness we find that owing to the immobility of the ear we can not locate sounds with precision. All are familiar with the difficulty of telling, especially in strange surroundings, whence a sound comes, unless the eye gives its help. The ringing of the bell of an unseen bicycle may cause us the most painful perplexity till we can learn its source by sight. Psychological experiments show in detail how untrustworthy are our attempts to localize sounds. Not that they are entirely untrustworthy. It may be that sounds have a special quality according to the direction from which they come and the way in which they strike upon the external ear; and recognition of this quality may give help towards their localization. But at the best, we are not freed from manifold confusions and errors. Thus it is found by experiment that while the change of position of the sounding body may be soon noticed, the direction of the change may be thought to be the opposite of what it is in reality. Again, relatively loud sounds are located preferably in front of the head, even when their source is

behind.* It can thus be seen that in accuracy of localization the ear can not be compared with the eye. The loss of the reflex apparatus by which the ear turned so as to catch most readily the vibrations in the air, has brought it about that the positions of sound are now so imperfectly apprehended by us.

The loss of this power of localization means two disadvantages. The first may be indicated in the words of Darwin:† "The power of erecting and directing the shell of the ears to the various points of the compass is no doubt of the highest service to many animals, as they thus perceive the direction of danger."

Closely connected with this practical disadvantage is another. The space of the ear has not the geometrical character of the spaces of sight and touch. Yet there is surely no good reason for doubting that it might have had much more of this character. Were the ear as mobile as the eye or the fingers, it would resemble them in the orderliness and well-defined character of the spatial forms it would yield. That its spatial form would equal in these respects that of the eye, it would be too much to affirm dogmatically. There may be more conditions to supply than merely that of mobility. Yet the touches from the less mobile parts of the body are singularly vague in their spatial outline as compared with those from the fingers and the tongue. And were the ear to gain mobility, we might expect to find it at least approximating, in its appreciation of form, to the senses which are regarded as so preeminently geometrical.

It is now apparent how serious are the disadvantages involved in the ear's immobility. Darwin thinks that the loss of the ear's movements is partly compensated by the increased ability to move the head about. It is true, these movements of the head are of importance both in seeing and in hearing. Yet in speaking of them as making up for the mobility of the sense-organs, we should be careful not to exaggerate their value. A man whose legs have been smitten with paralysis must find only small compensation for his affliction in the fact that a strong though somewhat slow porter is, when not otherwise occupied, ready to carry him about. It is also to be noticed that the eye has at its disposal the head movements, yet has retained its mobility.

We have now to ask what the mental gain is which has resulted from this loss. It is to be found in the ability to attend to a *succession* of sounds.

Let us notice how distinct is our perception of succession. A sound comes suddenly and sharply, and then it is gone, and another sound

* On the localization of sound, see 'Studies in Space Perception,' by A. H. Pierce.

† 'Descent of Man,' p. 14.

of distinct quality takes its place; thus by its very nature sound lends itself easily to this kind of perception. And when we listen to a sounding object, our interest is in catching the sounds which come in sequence. This is illustrated most distinctly, as we shall see, in attention to discourse. We hear simultaneous sounds, but the predominant characteristic of our perception of sounds is that their variety is given in a succession. Hearing is a time-sense. If the ear had remained mobile, it would have been the organ of a space-sense, for it would have given a number of sounds as practically coexisting and as coexisting in definite relations to each other; the mobility being lost, hearing has become a time-sense.

Contrast with the ear's perceptions those of the eye. We look at an object, and so long as we look, its form may remain the same. It may seem to be the same if we look at it after a day, or a month, or a decade. The great framework of our environment seems to the eye unchangeable.

It is not to be overlooked that we do perceive changes with the eye. We may watch a cloud melt in the summer sky, or we may call up the image of one who no longer lives. The eye can not ignore the fact of change, as the ear can not entirely ignore coexistence. And it is possible for us to school ourselves to note the changes from hour to hour in what we see. Yet the lesson is not naturally learned by the eye; its world is primarily a spatial world; its interest is in forms and the relations of these forms; for it succession is subordinate, as for the ear coexistence is subordinate. And as far as possible our idea of the stability of forms determines our interpretation of the changes we see. We watch a man walking along the street, or the trees waving in the wind. In such cases we see a change, but our mental reading of it is that a part of the spatial picture has been transferred from one point to another without any alteration in the intrinsic nature of the whole or the parts. It is possible that it is under the influence of the visual imagination that science keeps so persistently to the view that atoms shift their places, but do not suffer change. However this may be, the visual images of objects are spatial and in a measure stable; and they owe this peculiarity largely to the fact that the eye flashes from point to point and considers the external relations of figures to each other, to the comparative neglect of other aspects of reality.

On the other hand, the immobility of the ear contributes to the perception of succession inasmuch as the mind, being unable to get in simultaneity, or what is practically such, all the sounds of the environment, finds it easier to attend to the series of sounds. If nature had intended to cultivate the power of attending to a successive series of sensations, would not her first steps have been to make the organ

of these sensations stationary? Suppose the eye were to be trained to give special attention to the changes in objects before it, it would be essential that it should be prevented from making its usual excursions round the field open to it, and should be kept looking fixedly at one object. Not that this fixedness involves of necessity the inability to perceive a multiplicity of coexisting objects; it is found by experiment that when the eye is perfectly steady any one of the many points exposed to it can be attended to; and moreover, the attention can be directed from point to point. In hearing, too, we know that we can while remaining motionless, listen first to the sound from one quarter, then to that from another. But this only shows that when the natural instruments for performing certain acts are withdrawn from us, we may make shift to supply their places. We can see an object with the periphery of the eye, but we can not see it so well as when we freely turn the fovea upon it. And though we can direct our listening power from one point of the compass to another it remains true that the ear, smitten with immobility, can best fulfil its perceptive function when there is attention to the successive stimulations forming from one object.

It may seem that we have forgotten that such a sense as smell has an immobile organ, yet does not yield any special perception of succession. It is to be noted, however, that this sense is little developed in its perceptive aspect. We can not get the large number of discrete sensations from this sense that we can from hearing. We may by the ear distinguish five hundred sounds in the second. There is nothing in smell comparable to this. We need not wait to consider whether in its own undeveloped way smell does not after all remotely resemble hearing in the kind of perception it yields.

But we have not yet indicated the special forms assumed by the succession of sounds which it is so important to perceive. They are two—*language* and *music*. Language consists of a succession of sounds. When we consider how largely the intellectual life depends on language, we can see the enormous advantage of the development of the faculty of perceiving successive rather than simultaneous sounds. As every one is familiar with the importance of language, the greatness of the gain needs no further emphasis. Of less importance, though its significance for primeval man may yet prove to have been very great, is the appreciation of music. The music that is referred to is that given in melody. There is, apart from the melody, an appeal of each note and complex of notes which does not mean succession at all. Much of the thrill of music is an immediate effect of the individual note. But the appreciation of melody depends on the perception of succession. The eye is appealed to by a spatial combination of colors, the ear by a series of sounds. Readers of Lessing's *Laocoon* know how

finely he has elaborated the contrast between the esthetic characters of the two senses.

What, it will be asked, of the lower animals that have no external ear, or have one that can not be moved? In regard to such, we must carefully distinguish those species which have never possessed the movable ear from those which have lost the power of movement. It is the loss of a faculty once possessed that we are at present more immediately concerned with. Yet, in the case of such animals as the birds, which, though endowed with a highly developed sense of hearing, have no external ear, it is interesting to observe that there is remarkable appreciation of music. And this is not merely a response to individual sounds, as the musical appreciation of some animals may be; there seems to be an enjoyment of melody. Browning happily described the thrush as 'wise' because the bird 'sings each song twice over,' and thus shows his ability to 'recapture'

'The first, fine, careless rapture.'

It is also to be noticed that many birds can imitate other sounds, even those of the human voice. The repetition may be 'parrot-like,' but it gives evidence of the power of attending to a series of sounds.

It should be mentioned that the external ear of certain aquatic mammals is atrophied or lost. But as these animals have taken to a different kind of environment, and have been to so remarkable an extent made over, it seems unnecessary in the present investigation to give special consideration to this particular change in their structure.

The case of the monkeys seems at first to be different. Some of them, the anthropoid apes at least,* have like man lost the power to move the ears, yet they have not, it may be said, the faculty of speech. Have we not, then, the loss without any compensation of the special kind that is here being claimed for man? In considering this question we must keep in view the psychologist's ignorance of the mental life of the monkey. Notwithstanding all that has been written of the relationship of man to the monkeys, the psychology of these animals is still for the most part a blank. Yet there are some significant data that may in the present case be appealed to. The howling monkeys, though of low intelligence, find delight in the noise, from which they receive their name. They are gregarious and they howl in company. This noise is not made to drive away enemies; the monkeys gather deliberately for the purpose of making the noise and the leader starts the concert.

The chattering of monkeys should also be regarded as affording

* "The more recent ape ancestors, common to men and to the anthropoid apes (gorilla, chimpanzee, etc.) discontinued the habit of moving their ears and hence the motor muscles gradually became rudimentary and useless." Haeckel, 'Evolution of Man' (English Translation), Vol. II., pp. 270–271.

evidence of the appreciation which they have of sound. The chattering differs, moreover, from the mere monotonous repetition of a sound and if it has any function, it is probably a function which can be fulfilled only by the apprehension of a series of diverse sounds. It is also of interest to note the statement of Professor Haeckel that he has heard from apes of very different species 'remarkable clicking sounds'; and it has been thought that these sounds are still present in the language of Bushmen.

Mr. R. L. Garner made some years ago a study of the 'speech of monkeys,' and he reached the following conclusions:* "The sounds which monkeys make are voluntary, deliberate and articulate. They are always addressed to some certain individual with the evident purpose of having them understood. . . . They wait for and expect an answer, and if they do not receive one they frequently repeat the sounds. They usually look at the person addressed and do not utter these sounds when alone or as a mere pastime. . . . They understand the sounds made by monkeys of their own kind. . . . when imitated by a human being, by a whistle, a phonograph, or other mechanical devices. . . . The fundamental sounds appear to be pure vowels, but faint traces of consonants are found in many words." "As a rule each act of a monkey is attended by some sound." In a later work. Mr. Garner, after study of the apes in their native haunts, says that the chimpanzee has a vocabulary of twenty-five to thirty words; he claims that he learned ten of these words so that he could hold communication with the animals using them.†

Mr. Romanes'‡ account of the song, if such it may be called, of the chimpanzee 'Sally' may here be quoted: "It is sung without any regard to notation in a series of rapidly succeeding howls and screams —very loud, and accompanied by a drumming of the legs upon the ground." Mr. Garner has observed similar exhibitions given by chimpanzees. He also heard a performance of the kind in the African forest; the natives and others attributed it to the gorilla, but Mr. Garner thinks it not unlikely that it was given by the chimpanzee.

Darwin§ calls attention to the fact that two species of the gibbon, the *Hylobates agilis* and the *Hylobates leuciscus* have musical powers. In regard to the song of the former he quotes Mr. Waterhouse, who says: "It appeared to me that in ascending and descending the scale, the intervals were always exactly half tones; and I am sure that the highest note was the exact octave to the lowest. The quality of the notes is very musical."

---

* 'The Speech of Monkeys,' pp. 169–170.

† 'Apes and Monkeys,' p. 108.

‡ 'Mental Evolution in Man,' p. 377.

§ 'Descent of Man,' p. 567.

In considering the linguistic development of monkeys it is important to remember that monkeys have to a striking degree developed social qualities. Detailed proof of this sociality need not be given; its existence is known from the accounts of travelers, and of those who have domesticated these animals, and, indeed, from observations in zoological gardens. The knowledge of it is very incomplete; yet enough is known to show that it is often very intimate and not without complexity. Where there is such a social life, it is to be expected that there will be found a development in the use of sounds. Not that the presence of this development is hereby proved, but a presumption is created in favor of the view that it exists.* Darwin† thought that primeval man probably first used his voice in the production of true musical cadences, especially during courtship; and that the imitation of musical cries by articulate sounds might have given rise to words expressive of various complex emotional states. Should we not rather find the greater development of vocal signs in the apes earlier than primeval man which had variety of vocal utterance combined with the varied emotions of a complex social life, emotions not only of courtship, as Darwin supposes, but of parentage and of the various relations of friendliness and hostility?

It is not meant that all the monkeys referred to have the immobile ear. This is characteristic of the anthropoid apes. It is important, however, to observe in very diverse species of monkeys the peculiar interest in sounds; and in the anthropoid apes, which have lost the mobility of the ear, there is, as we see from the accounts of the gibbon and the chimpanzee, the special development of the use of, and appreciation of, vocal and other sounds.

It seems at first sight that the gain in the use of vocal sound made by the apes is too slight to account for the change in the organ of hearing. Yet we must hesitate to pronounce such a verdict when we consider the immense importance of any improvement in the faculty of language. Let an analogous case be considered. Mr. Fiske has shown that the slow growth of the brain is a condition of the attainment of the preeminent mental faculties possessed by man. This prolongation of infancy is in itself a disadvantage, but the gain resulting from it more than counterbalances the loss. But we find a similar slow growth in the case of the apes. Can we find in them any notable gain in intelligence? While they are intelligent animals, we can not appeal to a distinct and unchallengeable superiority. Nevertheless, we believe on evolutionary principles that there is a gain in mental faculty to warrant the slow maturing of their powers. Even so in the

* Mr. Garner claims that 'the more pronounced the gregarious habits of any species' of monkey are, the higher 'the type of speech it has.'

† 'Descent of Man,' p. 87.

case of language, we must say that any new form in the organism which conduced to the evolution of this faculty would be of such moment that, unless it entailed seriously deleterious effects, its permanence would be ensured.

To sum up, the loss of the ear's mobility has resulted in the fuller appreciation of the succession of sounds, and thus has been in an important sense a condition of the social, intellectual and esthetic development which has come with the use of language and music; and it is in a high degree probable, though the data are insufficient for conclusive demonstration, that it is to the advantage given in the struggle for existence by the first stages of this development that we are to attribute the permanent alteration in the structure of the ear.

We thus see that the sense organ having originally the form best adapted to the conditions in which the organism lived changed its form to meet the conditions of a higher stage of evolution. It may be that in this form it is most in accord with the special stimulations which appeal to it; it is certainly in this form that it can minister to the highest spiritual activities.

# SOME EIGHTEENTH CENTURY EVOLUTIONISTS.

By Professor ARTHUR LOVEJOY.
WASHINGTON UNIVERSITY.

A SATISFACTORY history of the theory of descent is a chapter in the records of human opinion that is still to be written. Meanwhile the subject is one about which persistent errors and illusions of historical perspective prevail. The popular mind appears to be firmly possessed by the belief that the doctrine of the evolution of species was a scientific innovation first promulgated, or at all events first cogently defended, by Darwin; the fame of the natural-selection hypothesis has become so great that its author figures, in the eyes of the great public, as the parent of the whole transformist system, while the earlier half century of controversy in behalf of that doctrine, under the leadership of Lamarck and of Geoffroy St. Hilaire, is forgotten. How far even instructed persons may suffer from this illusion of perspective was illustrated in the recent commemorations of the Emerson centenary. More than one of the eulogists of the great moralist of New England descanted upon his very un-Darwinian lines which tell how—

> striving to be man, the worm
> Mounts through all the spires of form,

as a remarkable 'anticipation of Darwin' and an example of the power of the poetic imagination to divine scientific truth. But the lines in question, added to the editions of Emerson's 'Nature' after 1849, are, of course, merely an epigrammatic versification of the main doctrine of Robert Chambers's 'Vestiges of Creation,' published in 1844; and the conception they express could hardly have been a very original one at any time after the appearance of Lamarck's 'Philosophie Zoologique' in 1809. The same confusion is illustrated again in the persistency with which writers on Tennyson take it for granted that the famous passage in 'In Memoriam' about nature,

> so careful of the type,
> So careless of the single life,

is an echo of the 'Origin of Species,' which in reality did not appear until at least fifteen years after this part of the poem was written. Even Mr. Frederic Harrison has—as Mr. Lang has pointed out—fallen into this error; and Mr. G. K. Chesterton has recently written about Tennyson in a way calculated to give the error fresh currency. But

even those who do not forget that the theory of the transmutation of species has been a familiar and influential doctrine, established upon fairly conclusive arguments ever since the beginning of the nineteenth century, are likely to forget the fact that the doctrine, in its proper modern form, takes its origin, as a respectably fathered and militant hypothesis, in France in the middle of the eighteenth century. The histories of the theory of evolution* mention, indeed, a number of names in the eighteenth and in many earlier centuries, with which vague and more or less eccentric foreshadowings of the now accepted doctrine are connected. But the books on the subject which we have in English are unfortunately either inadequate or inaccurate or both; and they rather disguise than reveal the real character and significance of the evolutionist movement in the eighteenth century. Many of them—Mr. Clodd's book, for example, and Huxley's essay, and Professor Packard's 'Lamarck,' as well as the French works of Perrier and of Quatrefages on the precursors of Darwin—ignore some of the most important and most influential eighteenth century evolutionists; Professor Osborn's survey ('From the Greeks to Darwin,' 1894) is more comprehensive but regrettably inaccurate. There is therefore some occasion for a fresh attempt to clear up some points in the earlier history of the central conception of modern biology.

It is unfortunate that the eighteenth century manifestations of evolutionism should have so generally been grouped, by those who have written of them, in one class with the ancient adumbrations of Darwinism, as if all alike were merely interesting historic accidents. The ancient foreshadowings of the doctrine were, indeed, little more than happy but fortuitous guesses of ingenious minds. But the mid-eighteenth century outcropping of the theory was a natural, one may almost say an inevitable, consequence of the progress which had up to that time been made in natural science. And the theory found expression, not in the sporadic utterances of an obscure philosopher here and there, but in the best-known writings of three of the most celebrated leaders of the opinion of their time; so that, however little it may have gained acceptance, the theory must have been pretty widely known among their contemporaries. It is, of course, a fact sufficiently familiar that Buffon in 1749 propounded the conception of the transformation of species as a possible hypothesis; that he pointed out the homological evidence in favor of such an hypothesis, and tended in some passages to accept it; but that, in his most important passage on

* In this paper the word 'evolution' is used in its common contemporary sense, as meaning the descent of species from earlier species. The reader will, however, remember that in the eighteenth century the same term was employed to designate the process of the generation of individual organisms as conceived by the preformationist,—*i. e.*, the process of the literal 'development' or unfolding of the ready-made and preexisting parts of the embryo. Most 'evolutionists' in this eighteenth century sense were not evolutionists in the more modern sense in which the word is here used.

the subject, he rejected it, partly on grounds of religious orthodoxy. Professor Packard, in his life of Lamarck, has recently presented an interesting study of Buffon's exact position in the matter. These equivocal expressions of Buffon's are, however, commonly spoken of as if they were unique in their period; whereas the same hypothesis was put forward, within a decade, by two countrymen of his who were hardly less representative than he of the scientific progress of their generation. For one of them was the president of the Royal Academy of Science of Berlin; and the other was the editor of the Encyclopædia.

There were two distinct lines of development in scientific investigation and theory during the first half of the eighteenth century which led up to and suggested the theory of transformation as a natural and probable hypothesis in zoology. The first of these was the active prosecution of both observation and speculation in the field of embryology; the second was the development of the new science of comparative anatomy at the hands of Daubenton. The representative of the former way of approach to evolutionism is Maupertuis. In speaking of him, I venture to improve the occasion to give some general account of his place in the history of science, since the matter is one about which little trustworthy information appears to be generally accessible. Such an account will make the significance and the grounds of his evolutionary opinions more apparent.

I. *Maupertuis.*—Although not without some reputation as the reorganizer of the Berlin Academy*—for which task he was especially imported from France by Frederick the Great—and as the director of the first expedition to demonstrate the flattening of the globe at the poles by the measurement of a degree of longitude at different latitudes, Maupertuis is usually made to play a somewhat comic rôle in the literary history of his century, as the rival of Voltaire for the favor of Frederick and as the victim of one of Voltaire's most ferocious satires. Although Frederick took the side of Maupertuis in that famous quarrel and caused the copies of Voltaire's libel to be burned by the hangman in all the public places of Berlin, the satirist has been more successful in gaining the ear of posterity. Immensely famous and respected as a sort of scientific oracle in his own day, Maupertuis seems now to be best known through the misrepresentations of his adversary; there is even reason to fear, from internal evidence, that some learned historians of philosophy, in the little they have to say about the 'Native of St. Malo'—as Voltaire always designated him—have depended more upon the 'Histoire du Docteur Akakia' than upon a careful examination of Maupertuis's own writings. Yet—in spite of the touch of vanity which sometimes made him ridiculous and the superficiality of a good deal of his knowledge—his reputation deserves in some measure

* For the earlier history of the Berlin Academy, see *The Popular Science Monthly*, March, 1904.

to be rehabilitated. He was by no means a great scientific investigator; his work in physics and in astronomy, which he professed for his chief specialties, seems to be of decidedly questionable accuracy and value. His celebrated 'law of least action,' which was the original occasion of his quarrel with Koenig and Voltaire, was a generalization vaguely conceived and ill formulated, although, as Mach has pointed out, it was taken up by Euler, the friend and partisan of Maupertuis, and transformed into an important physical principle. But in any history of the general movement of scientific thought in his century Maupertuis clearly merits a place of some distinction. For he was the possessor of a wide view of the interrelation of different scientific problems; he was an ingenious and yet often a pretty shrewd and critical interpreter of the bearing and ulterior consequences of the scientific discoveries of others; and he contributed to more than one branch of science new and important conceptions, which during the subsequent century and a half have come into great vogue and in some cases into general acceptance.

As was the fashion of his time, Maupertuis took philosophy as well as physical science for his province; and before considering his work in the latter domain, it is worth noting that in the former also he was the proposer of several notions, now familiar enough, which were at the time relatively novel and contrary to the prevailing intellectual fashions. As a moralist, for example, Maupertuis raised a question that has been repeated with much doleful iteration by his nineteenth century successors, but one highly paradoxical to his contemporaries: In ordinary human life, does the sum of dissatisfactions exceed the sum of pleasures?* This question he answered in a pessimistic sense, in an age when a superficial optimism was the proper note among enlightened *philosophes*—and was reproached for it by the writer who, a few years later, was to produce the 'Poem on the Lisbon Disaster' and 'Candide.' In laying down the logical conditions for dealing with such a question, Maupertuis anticipated Bentham and the 'moral arithmetic' of the Utilitarians, by elaborating a species of hedonic calculus, in which careful definitions are offered, not only of the nature of pleasure and pain, but also of the several dimensions of each that must be reckoned in assessing the relative value of any two 'sums of pleasure,' or of its contrary. As a political theorist, also, he shows himself a precursor of the English Utilitarian school, at a time when nearly all the new systems of political philosophy were based upon some form of the conception of 'natural rights' or 'natural law.' In his 'Eloge de M. de Montesquieu' he criticizes the political doctrine of the 'Esprit des Lois,' which rests, he says, upon the assumption that there inheres in human relations 'un certain rapport d'équité' which man's reason immediately recognizes. "It is not," writes Maupertuis,

* 'Essai de philosophie morale,' chaps. 1 and 2.

"such a principle as this that should be accepted as the fundamental principle of legislation; this is too obscure, too vague, too susceptible of different interpretations; it would leave too much to the arbitrary judgment of the legislation." The only safe guide in legislation is the *principe du plus grand bonheur.* "The problem of the legislator is simply this: A multitude of men being collected together, to procure for them the greatest sum of happiness possible. It is upon his principle that all systems of legislation should be based." All this reads like so many sentences from Bentham himself; and the resemblance is by no means merely coincidental. In the ethical and political writings of Maupertuis and of Helvétius we have the head-waters of the important stream of utilitarian influence which became so broad and sweeping a current through the work of the Benthamites. Bentham read Maupertuis early—perhaps about 1770, in his twenty-second or twenty-third year, thinks a recent writer on the subject*—and although he had already got the suggestion of his doctrine from Priestley and Helvétius and Beccaria, he found, as he himself tells us, his utilitarian tendencies strengthened and corroborated by his reading of the 'Essai de Philosophie Morale.'† The utilitarian political teaching of Maupertuis was enunciated at least three years before the publication of a similar doctrine in the book of Helvétius (*De l'Esprit,* 1758); and that book, Mr. John Morley has said, 'contained the one principle capable of supplying such a system of thinking about society as would have taught the French of that time in what direction to look for reforms.' The work of Beccaria, the third of the early influences upon the mind of Bentham, was still later in date of publication (1764).

In treating of the relation of scientific method to theology, Maupertuis—although professing a somewhat perfunctory religious orthodoxy—criticized the favorite eighteenth century argument for theism —the so-called argument from design—in which the deists no less than the orthodox of the period found the principal basis of their religious philosophy; and his criticism upon it is just such as a Darwinian might now make. It closely resembles, indeed, the criticism of the same argument that Romanes put forward long afterward as a special outcome of Darwinism.‡ Many, says Maupertuis, have found an evidence of design in the marvelous adaptation of the organs of animals to their needs. But "may we not say that, in the fortuitous combination of the productions of Nature, since only those creatures *could* survive in whose organization a certain degree of adaptation was present (*ou se trouvaient certains rapports de convenance*), there is nothing extraordinary in the fact that such adaptation is actually

* Halévy, 'La jeunesse de Bentham,' 1901, p. 288.
† *Ibid.*, p. 406.
‡ 'A Candid Examination of Theism.'

found in all those species which now exist? Chance, one might say, turned out a vast number of individuals; a small proportion of these were organized in such a manner that the animals' organs could satisfy their needs. A much greater number showed neither adaptation nor order; these last have all perished. . . . Thus the species which we see to-day are but a small part of all those that a blind destiny has produced." Maupertuis did not dogmatically maintain the anti-teleological position which this criticism tended to justify; he only maintained that zoology can not assist theology, because the former has no need of teleological explanations and can sufficiently account for the degree of adaptation which exists on the principle which we should now call that of the survival of the fittest, *i. e.*, of the best adapted.

Maupertuis had also his own theories in metaphysics; but these are so closely connected with his evolutionary views that the two should be considered together. I turn, then, to mention his work in promoting new ideas in natural science. He was the first to introduce the Newtonian physics and astronomy into France. In the face of a good deal of opposition, he successfully disseminated the doctrine of attraction among the learned; and it was apparently from him that Voltaire acquired a sufficient smattering of physics and astronomy to enable him to write his 'Eléments de la Philosophie de Newton' (1738). As became the president of an academy, Maupertuis undertook, in his 'Lettres' and 'Lettres sur le Progres des Sciences'* to sum up certain of the most important gains that had been made by scientific inquiry, and to lay down a program of experimental investigations next to be undertaken. These investigations, he urged, should be supported by the state, when they are too elaborate or too expensive to be undertaken by private enterprise. Some of his suggestions are pretty fantastic and impracticable; but the greater number show good sense and a keen appreciation of the importance of systematic experimentation, even in sciences where experimental methods had as yet been little used. He recommends, among other things, the exploration of the north and south polar regions, and of the interior of Africa, for the settlement of the chief unsettled questions in geography; urges the employment of experimental methods in zoology, especially in the study of the problems of heredity; advises specialization in medical

* 'Oeuvres,' 1756, tome II. The proposals contained in these letters were the special objects of Voltaire's ridicule. But—M. Desnoiresterres ('Voltaire et Frédéric,' ch. 8) to the contrary notwithstanding—Voltaire gains nearly all his effects either by deliberately misrepresenting Maupertuis, or by presenting as absurdities ideas which to the unprejudiced will rather seem evidences of soundness of judgment. The Kantian idealist of our time, for example, will find some lack of point in this attempt at the ironical: 'Le candidat (Maupertuis) se trompe, quand il dit que l'étendue n'est qu'une perception de notre âme. S'il fait jamais de bonnes études, il verra que l'étendue n'est pas comme le son et les couleurs, qui n'existent que dans nos sensations, comme le sait tout écolier.'

practise; proposes the utilization of the bodies of condemned criminals for experiments on the etiology of disease; calls for the prosecution of systematic experiments with electricity, and the abandonment of premature efforts to make practical use of that force, before its properties and behavior had been adequately investigated; and indicates the possibility of the prosecution of certain *expériences métaphysiques*—*i. e.*, of investigations in what we should now call experimental psychology. He concludes his program somewhat humorously with an enumeration of *récherches à interdire*—namely, those 'chimeras of science,' the philosopher's stone, the quadrature of the circle and perpetual motion. In regard to the first of these, however—the transmutation of elements—he points out that the thing can not be shown to be inherently impossible. For there are several legitimate hypotheses about the constitution of matter which are compatible with the possibility of transmutation. It is not unlikely, for example, that 'matter is composed of homogeneous parts,' and that the elements which appear to possess irreducible qualitative differences, 'really differ from one another only by the dissimilar form and arrangement of the homogeneous particles which compose them.' In that case, we should not be entitled to declare it impossible to give 'to such particles a different form and arrangement, which is all that would be necessary in order to transmute lead or wool into gold.' The objection to the search for the means of transmuting elements is, therefore, not that it can be demonstrated to aim at the impossible, but only that in the existing state of science, the value of the goal—great as it would be—'is not great enough to counterbalance the scant probability of attaining it.' When one reminds oneself of the hypotheses about the constitution of matter that have come into especial vogue since the discovery of the properties of radium, these observations strike one as the expression of a rather well balanced judgment.

It was, however, in his conception of the methods and the possibilities of natural history that Maupertuis most evidently showed himself the possessor of a wider intellectual horizon than was common among the men of science of his time. Zoologists had as yet seen little occasion to attempt more than the careful description and classification of animals; but to Maupertuis a purely descriptive and classificatory science, which was unable to formulate any laws concerning the *processes* going on in that part of nature with which it dealt, was, strictly speaking, no science at all. He had little patience with naturalists whose view of their province was so narrow. 'All these treatises on animals which we as yet have,' he writes in the 'Lettres sur le Progrès des Sciences,' 'are —even the most methodical of them—no better than pictures pretty to look at; in order to make of natural history a veritable science, naturalists must apply themselves to researches which can make us acquainted, not simply with the form of this or that animal, but with the general

processes of nature in the production of animals and the conservation of them.' The general processes which Maupertuis thought it especially important that zoological science should investigate are those through which animal individuals and species have come to have the differences of form and function that distinguish them. Maupertuis, in a word, appears to have clearly envisaged the genetic problem in biology, at a period in the history of thought when genetic problems generally were little considered. The center of interest in zoology therefore lay, for him, in the problems of embryogeny and of heredity. Although not himself an anatomist, he made himself familiar with investigations made by others on the minute anatomy of the embryo. And, as I have intimated, he never tired of insisting that the facts of heredity should be investigated, in the case of animals, by experiments in the interbreeding of species and varieties, and, in the case of human beings, by a collation of family histories.

The opinions of Maupertuis on these matters are expressed chiefly in the work called 'Vénus Physique' (1745) and in the 'Système de la Nature' (1751). The latter first appeared in the form of a Latin dissertation ostensibly delivered at Erlangen by one Dr. Baumann. Maupertuis found it expedient thus to shelter himself against reproach on account of any heterodox tendencies that the book might be found to contain. Four editions—all but the first in French—were called for within four years, and the author soon assumed responsibility for his work. In the 'Vénus Physique' Maupertuis essayed the popular style, and the book is consequently marred by passages written in an abominably rhetorical and affected manner. But, none the less, it constitutes, if I am not mistaken, the first important attack made in the eighteenth century upon the theory of the preformation of the embryo. Harvey had advanced the doctrine of epigenesis nearly a century earlier, but his arguments had failed to convince his successors, and his observations upon the chick had been shown by Malpighi to be partially erroneous.* At the time when Maupertuis wrote, preformationism had long been the ruling doctrine in embryology; an immense weight of scientific authority was in its favor. Among the philosophers Malebranche and Leibniz had argued for it, among the great physiologists and anatomists Swammerdam, Redi, Malpighi, Leeuwenhoek, Winslow and Haller had taught it. Bonnet was yet to give it its most elaborate exposition and defense; and three quarters of a century later it was still to find an adherent in Cuvier. The 'Vénus Physique' is a review of the preformation theory in its several forms, designed to show that the evidence against it is conclusive.

Of the arguments for epigenesis which Maupertuis offers it is not possible, in this brief paper, to give any sufficient account. He relies in part upon the observations of Harvey—and in so doing shows himself

* 'De formatione pulli in ovo,' 1673; 'De ovo incubato,' 1686.

not quite abreast of the anatomical knowledge of his time, since, correct as Harvey's main conclusions were, the observations upon which he based them had already been superseded. Chiefly, however, Maupertuis rests his case upon the facts of double heredity and hybridism. If the embryo be truly 'predelineated' in the ovum or in the 'zoosperm,' how, he asks, can it come about that it inherits the specific or individual characters of now one and now the other parent, and often of both? The preformationists had, of course, their devices for explaining away this pretty obvious difficulty; but Maupertuis finds it easy to show that the explanations are altogether inadequate when they are compared with even the common and easily observable facts of heredity. His reasoning is especially effective when he cites his own investigation of the transmission of hexadactylism (*sexdigitisme*) through several generations of a certain German family whose records he had examined, and points out how little the preformation hypothesis could account for the transmission of such a peculiarity through male and female parents alike, its progressive disappearance as succeeding generations more and more intermarried with persons having the normal number of digits, and its occasional atavistic reappearance in remote descendants. In view of these classes of facts, he declares, the encasement theory must be abandoned, and the conclusion must be accepted that the embryo is no ready-made article, preexistent from the creation of the world, but a new birth, the product of a true genesis;—not, indeed, a genesis of life itself, but of a new and unique combination and intermingling of already-living elements contributed by both parents alike. These arguments and this conclusion, it should be remembered, were advanced by Maupertuis more than a decade before the publication of the great work of Kaspar Friedrich Wolff,* from which the modern revival of the doctrine of epigenesis is usually dated. The conception of epigenesis held by Maupertuis, was, moreover, far more complete and accurate than that which Harvey had put forward a century earlier. For although Harvey had asserted *marem et foeminam pariter efficientes causas esse generationis,* he had denied that there can be any physical interpenetration of ovum and spermatozoon, and had declared that fecundation consists in the communication of a purely immaterial force. It was in order to make this a little more intelligible that Harvey had worked out his famous analogy between the conception of the embryo in the uterus, and the conception of an idea in the brain.† All this, Maupertuis remarks, is an *idée étrange;* where we have double

* 'Theoria Generationis,' 1759.

† Harvey's reason for this opinion lay in his failure to discover any traces of the spermatozoon in the uterus. His own words are 'Quoniam nihil sensibile in utero post coitum reperitur; et tamen necesse est ut aliquid adsit, quod foeminam foecundam reddat; atque illud, ut probabile est, corporeum esse nequeat: superest ut ad merum conceptum, specierumque sine materia receptionem, confugiamus,' *i. e.*, the ovum is fertilized by being impregnated with a general concept! ('De generatione animalium,' 33).

heredity we must assume a communication of corporeal elements, vehicles of that heredity, from both sides. And, as Maupertuis also observed, the supposed fact by which Harvey had justified this singular conclusion had already been rendered more than questionable by later investigations of Verheyen.

Having established these essential principles to his satisfaction, Maupertuis proceeded to formulate an hypothesis concerning the nature of the fundamental physical process presupposed by the facts of heredity, on the one hand, and of variation, on the other. The formation of an embryo must, he conceived, be due to the combination in a new organic union, of a great number of living corporeal particles, derived usually from both parents, each of which particles carries with it a sort of organic memory (*souvenir*) of the life of the organism to which it formerly belonged, and thereby tends to unite with the other particles in such a way as to produce a new organism of the same species.* This process of recombination of already living particles was held by Maupertuis to be governed by something more than the laws of mechanism; embryogeny was for him no mere process of juxtaposition, under the laws of gravitation, of so many inert atoms; the ultimate units of the newly constituted living being all possess their own self-contained law of development, and their own distinctive selective affinities for certain other units. None the less, purely mechanical displacements of parts also take place; and to these in part, he supposed, is due the occurrence of monstrous forms, and many of the more ordinary variations from the hereditary specific type. Moreover, the elementary units which, coming from the parents, combine to form the embryonic offspring, in part carry with them a similar sort of organic memory of the particular and individual characters of the parent, and so tend to develop those characters in that offspring; but in part also they are free from this tendency, and carry with them rather the traits of more remote ancestors (atavism), and some of them may even be wholly independent of hereditary predetermination. It is these especially which, in Maupertuis's hypothesis,† constitute the explanation of the general tendency to variation in animals, which he recognized to the full. If one must have a further explanation why the transmitted corpuscles tend to reproduce the characters of the parents, Maupertuis suggests that perhaps there enters into the embryo a separate germ from each part of the body of the parent of which the character is reproduced; in other words, he proposes a hypothesis similar to the Dar-

* 'Vénus Physique,' Pt. II., ch. 5. We must suppose 'que la liqueur séminale de chaques espèce d'animau, contient une multitude innombrable de parties propres à former par leurs assemblages des animaux de la même espèce.

† *Loc. cit.* It is likewise to be assumed 'que dans la liqueur séminale de chaque individu, les parties propres à former des traits semblables à ceux de cet individu sont celles qui d'ordinaire sont en plus grand nombre, et qui ont le plus d'affinité; quoiqu'il y en ait beaucoup d'autres pour des traits différents.'

winian theory of Pangenesis. All these ideas are put forward by Maupertuis only as so many likely explanations of facts which, as he insisted, needed more adequate analysis and explanation than the embryological doctrines of the time afforded; his pangenetic theory, in particular, he regarded only as *une conjecture bien hardie, mais qui ne serait peut-être pas destituée de toute vraisemblance.*

What is noteworthy in these hypotheses is the group of truths which they involve incidentally. From Maupertuis's exposition of them it is clear that he had been led, by his reflections upon the facts of heredity, to recognize (*a*) that there is a constant tendency to variation in animals by reason of their double heredity; (*b*) that there is a further tendency to spontaneous and accidental variations, due in part to mechanical displacements or chance combinations among the ultimate particles of which the embryo is composed; (*c*) that these variations —and possibly also new characters acquired during the lifetime of the parent*—tend to be perpetuated through heredity, provided that they do not unfit the animals that possess them for survival in their environment, and provided also that they are not gradually obliterated through inter-breeding with animals that do not possess them. To one who thus emphasized the factors making for variation and for the conservation of variations, the theory of the mutability of species necessarily appeared more natural than the theory of their fixity. Maupertuis thus passes at once from his theories of heredity to propound the hypothesis that all species may have come from a single primitive pair through the gradual accumulation and transmission of divergent variations. Granting these facts about variation, he writes, "would it not be possible to explain by means of them how the multiplication of the most dissimilar species might be traced back to (*aurait pu s'ensuivre de*) only two individuals. Such species would have owed their origination merely to the accidental production of certain embryos (*à quelques productions fortuites*) in which the elementary parts had not retained the arrangement which they had had in the parent animals. Each degree of deviation (*erreur*) would bring about a new species; and by means of repeated departures from the original form (*à force d'écarts repétés*) there would have come about the infinite diversity of animals that we see to-day:—a diversity which may in time increase still further, but to which it may be that the lapse of centuries will bring only imperceptible additions" ('Système de la Nature,' XLV.). In the 'Lettres' Maupertuis again writes, *à propos* of the inheritance of certain congenital individual variations in the human

* Maupertuis raises the question concerning the inheritance of acquired characters, but suspends judgment upon it, and calls for further experimentation. 'Ce serait assurément quelque chose qui mériterait bien l'attention des philosophes, que d'éprouver si certaines singularités artificielles des animaux ne passeraient pas, après plusieurs générations, aux animaux qui naîtraient de ceux-là.'

species: "I hold that these supernumerary digits are, at their first appearance, nothing but accidental variations. . . . But these variations once well established (*confirmées*) through a sufficient number of generations in which both sexes have had them, constitute (*fondent*) species; and it is perhaps thus that all species have multiplied." In fact, for Maupertuis the difficulty lay in explaining, not how species are transformed, but why they are so stable.

It will be seen that Maupertuis puts forward his theory of transformation only as a likely hypothesis, not as a settled truth. But it is an hypothesis for which he clearly enough indicates his own preference; and it is certain, from a passage of Diderot's which I shall presently quote, that his contemporaries looked upon him as the typical representative of the doctrine of the descent of all species from a primitive type. Yet the significance and originality of the work of Maupertuis lie not so much in his explicit enunciation of the theory of descent, as in the fact that he (1) insistently called the attention of naturalists to the problems connected with the genesis and transmission of variations; (2) framed a conception of the processes involved in embryogeny and heredity which made the mutability of species seem antecedently the more natural and more probable hypothesis; (3) indicated a program for systematic observation and experimentation with reference to heredity and to the effects of interbreeding, which, if carried out, would have transformed zoology; (4) intimated that Nature produces a far greater number of types and of individuals than she can maintain, and that among all these variant types there is constantly taking place a process of natural selection whereby those unfitted to the conditions of their life are exterminated; (5) explained the adaptation of animals to their environment solely as the result of these conjoint processes of variation and selection.*

* Professor Osborn, unlike most of the historians of evolutionism, makes some mention of Maupertuis, but classifies and describes his doctrines in a very curious fashion. He classes the president of the Berlin Academy, as well as the editor of the Encyclopædia, with such 'evolutionists' as de Maillet (who 'derived man from *l'homme marin*, the husband of the mermaid'), and Duret (who asserted that there were trees in Scotland, the leaves of which, falling on one side into the sea, became fishes, and falling on the other side on land, became birds). Of all these equally Professor Osborn says: "They were not actually in the main evolution movement; they were either out of date or upon the side-tracks of thought. They can be sharply distinguished from both the naturalists and the philosophers in the fact that their speculations advanced without the support of observation, and without the least deference to inductive canons." Such a characterization, applied to men like Maupertuis and Diderot, certainly fails somewhat in deference to the ordinary canons of historical accuracy. Professor Osborn mentions, indeed, that 'an obscure article' (the 'Système de la Nature) by Maupertuis 'has been unearthed in the course of the present diligent search for all the prophecies of evolution,' and a partially correct account is given of some of the contentions of that writing. But no clear indication is given of the grounds of the evolutionism of Maupertuis; and the writer of 'From the Greeks to Darwin' appears to have been unacquainted with the 'Vênus Physique' and to have ignored the work of Maupertuis in the rehabilitation of the doctrine of epigenesis. He implies also that Buffon's theory

It remains to point out that the embryological doctrines of Maupertuis were intimately connected with certain metaphysical conceptions, of a type that has often since shown itself to be peculiarly congenial to minds trained in biology. The 'Système de la Nature' is primarily an exposition and a defense of the theory that all matter possesses some sort of consciousness—a 'monism' similar to that of which Haeckel is the contemporary prophet. A purely mechanistic materialism, such as the atomism of the Epicureans, or the crude notions which La Mettrie had recently put forward, seemed to Maupertuis an evident absurdity; 'in order to overthrow such a system,' he writes, 'one need do no more than ask those who hold it how it would be possible for atoms without intelligence to produce an intelligence.' Mere mechanism appeared as little capable of explaining the phenomena of organic life, as it was of explaining the phenomena of consciousness; especially manifest, Maupertuis thought, is the inadequacy of purely mechanical causes to account for the processes which he conceived to be involved in the formation of the embryo. "A blind attraction uniformly distributed throughout all particles of matter can not serve to explain how these particles arrange themselves to form even the simplest of organized bodies. If they all have the same tendency, the same power, to unite with one another, why is it that certain elements go to form the eye, certain others to form the ear, etc.? Why this marvelous arrangement? Why is it that the various elements are not united pell-mell?" The combination of material particles to form a living organism seemed to imply a principle of selection, a species of elective affinity between the particles, which could not be reduced to physical or chemical categories; and the singular fact of heredity, the transmission of qualitative similarities from one organism to another through whatever minute bodies serve as the vehicles of heredity, seemed to imply the possession by those bodies of something which could only be conceived under the analogy of conscious memory. It is necessary, then, to attribute to each particle of matter the possession of some rudimentary forms of sentiency, memory and volition. (*Si l'on veut dire sur cela quelque chose qu'on conçoive, quoiqu'encore on ne le conçoive que sur quelque analogie, il faut avoir recours a quelque principe d'intelligence, a quelque chose de semblable a ce que nous appellons désir, aversion, mémoire*). Maupertuis does not forget the radical difficulty which has been urged against the identification of the *res cogitans* and the *res extensa* ever since Descartes—the difficulty, namely, that the attributes of consciousness and extension have nothing in common, and that neither can thought be conceived as ex-

of generation appeared earlier than that of Maupertuis, which is not the case. The general conception of the 'evolution movement' and of the relative importance of its several eighteenth century representatives, in Professor Osborn's book, seems to the present writer decidedly misleading.

tended, nor extended matter as possessing the unity characteristic of conscious thought. Maupertuis certainly can not be said to meet this difficulty; but he evades it by a device which has been much employed since his time by metaphysicians of opinions kindred to his. The objection in question, he avows, would be a legitimate one against any doctrine that actually asserted the identity of matter and consciousness, reducing matter to thought, or thought to a form or function of matter. But if we say that thought and extension are not things, but properties—distinct but joint properties of a common subject—the difficulty, he contends, disappears. This *tertium quid,* of which thought and extension are to be defined as coexisting properties, is something 'of which the essence is unknown to us' ('Syst. de la Nat.,' 22). Maupertuis at this point appears, on the one hand, as repeating the dialectical strategy of Spinoza, a philosopher almost wholly ignored in the eighteenth century; and, on the other hand, as a precursor of Mr. Herbert Spencer, with his conception of the 'double aspect of an ultimately unknowable substance.' Maupertuis, however, was not a psychophysical parallelist; on the contrary, as I have pointed out, the sentiency which he attributed to matter was regarded by him as an essential factor in the explanation of physical events.

How Maupertuis would have reconciled the apparent—even though 'transfigured'—realism of this doctrine of conscious matter with the idealistic view of the subjectivity of the perception of space, which he expresses in one of the Lettres,* it is impossible to say. It may be that it never occurred to him that the two opinions were discrepant; it may be that he conceived it possible to reconcile them; and it may be that the idealistic view, which was published later than the other, implied the abandonment of the realism of the mind-stuff theory. As it is, we can only say that, as a metaphysician, Maupertuis has the apparently contradictory distinction of having given utterance, during the middle decade of the eighteenth century, to the favorite contentions of both the realism and the idealism of the nineteenth.†

---

* Lettre IV. 'Sur la manière dont nous aperçevons.' For Voltaire's comment on this, see above, p. 9, footnote. Maupertuis expresses this idealistic conclusion in these terms: 'Refléchissant donc sur ce qu'il n'y a aucun rapport, entre nos perceptions et les objets extérieurs, on conviendra que tous ces objets ne sont que de simples phenoménes: l'étendue, que nous avons prise pour la base de tous ces objets, pour ce qu'en concerne l'essence, l'étendue elle-même ne sera rien de plus qu'un phenoméne.'

† Historians of philosophy have unduly neglected both aspects of Maupertuis as a metaphysician. Lange merely mentions his doctrine of 'empfindende Atome' in a sentence ('Gesch. d. Materialismus,' I., 259); Erdmann, who devotes a page to Maupertuis, says nothing about his metaphysics at all ('History of Philosophy,' II., 293, 4).

*(To be continued.)*

# SALT.

By Professor CHARLES W. SUPER,

ATHENS, OHIO.

EVERYBODY knows the lines in Lucile in which the author declares that 'civilized man can not live without cooks.' He also proposes the query whether there is any man in the world who can live without dining. The assertion is true only with important restrictions; for it will not be contended by anybody that every person who cooks is a cook, any more than it would be affirmed that every one who paints is a painter. The interrogatory may be frankly answered in the negative, since the great majority of mankind does not now dine and never has dined. They eat when they have food, and when they have none they do without. If we call this spasmodic way of supplying the interior department with materials for slow combustion, quaddrupeds may be said to dine with as much propriety as *homo erectus*. If our poet had asked the question, Where is the man, civilized or uncivilized, who can live without salt? every one of his readers would probably have replied unhesitatingly, 'He does not exist.' It is doubtful too whether he ever existed. It is asserted by competent authorities that terrestrial as well as marine life is conditioned upon the consumption of salt. The position is hard to prove or disprove, as experiments that would give trustworthy results are almost impossible. It seems, however, fairly well established that man at the present day, no matter what his rank on the staircase of social progress, can not or, at least, does not, live without this substance. What history has to say will be given below. That a historical record and an established fact are not interchangeable terms is, however, to be premised. Not only has this mineral been found in close proximity to almost every locality inhabited by man or at least within his reach; it is sought with almost equal avidity by brutes. Most domestic animals are particularly fond of it. It is said to be fatal to some kinds of birds, though barn-yard fowls consume it without injury. The herbivora have an especial liking for it, whether in their wild state or domesticated. It is well known that the various salt licks in the United States were favorite places for ambuscades, and that both Indians and whites used them for the purpose of destroying the deer, buffalo and other animals that habitually resorted to them. Probably the most famous of these salt springs, or licks, as they are generally designated, is the Big Bone Lick

in Boone County, Kentucky. Professor Shaler in his history of the state says:

> Not only do we find the bones of animals which occupied the country when the whites first came to it—the buffalo, the elk, the deer, etc.—but also deeper in the mire, or in portions that indicate a greater antiquity, great quantities of the bones of the fossil elephant, his lesser kinsman the mastodon, the musk-ox, an extinct long-legged buffalo, the caribou or American reindeer, and various other creatures which dwelt here in the time when the last glacial period covered the more northern regions with a mantle of ice.

The number of animals buried in the swampy soil about this lick is enormous. Many of them, in their eagerness to get at the brine, rushed beyond their depth, and before they were aware of it were borne down by their own weight until they were unable to extricate themselves, and so died of starvation. Others were probably pushed forward by those that crowded on from behind and trodden into the soft earth, where they died of suffocation. The locality was equally fatal to small and to large animals. How many years or cycles ago this destruction began we have no means of knowing, but that it continued to comparatively recent times is extremely probable.

Let us now examine some evidence which goes to show that man has lived without salt. Sallust in his 'History of the Jugurthine War' says the Numidians live chiefly on milk and the flesh of wild animals, and that they use no salt or other relishes. Not only is the time to which the historian refers comparatively recent, but he has the reputation of carefully verifying his facts. His statements, therefore, carry great weight. It is held, moreover, that the Finnish name for salt is derived from an Indo-European root. If this view is correct the inference is natural and legitimate that the Finns did not know this commodity until they came in contact with Aryans, probably Slavs, from whom they got both the name and the thing, or rather the thing and the name by which they heard it called. In the Odyssey the renowned seer, Teiresias, directs Ulysses to travel until he comes to 'men who know not the sea neither eat meat flavored with salt.' Pliny supposes the Epirotes to be meant by this passage. But the point of chief interest is that to the Homeric Greeks a saltless people were supposed to live somewhere in the interior and in the most primitive condition. The poet, instead of naming a dozen points of difference, with epic prolixity, in life and usage between his own nation and this far-off tribe, has selected a single characteristic as sufficiently explicit for his purpose. Tacitus relates that toward the close of the first christian century a great battle was fought between the Hermanduri and the Chatti for the possession of a river boundary, a salt-producing stream, because both parties believed that at this place heaven was especially near and that nowhere else could they address their prayers to the gods

in such close proximity. There is reason to believe that this river was the Werra. On its banks, near the town of Salzungen, saline springs have been known from time immemorial and are still in use. The historian further relates that salt was produced near the river and in the contiguous forest, not, as elsewhere, by the evaporation of sea-water, but by pouring brine over a pile of burning wood, with the result that the salt was precipitated as a consequence of the struggle between the two elements, fire and water. Evidently the sacred character that was supposed to attach to this saline substance was due to the belief held by the natives that salt was always a product of the sea, except by the special interposition of the gods, as in this case. That they had contracted a liking for salt elsewhere in their wanderings may be taken for granted.

Salt is now produced in many parts of Germany, but its existence in any form was not known at this remote period. The article produced in such a singular manner must have been very impure; but the palates of the primitive Germans were much less sensitive than those of their modern successors. At a later period the Alemani and the Burgundians are said to have frequently striven in battle for salt pits or saline springs claimed by both; but the region can not be definitely located. The record is chiefly interesting when taken in connection with the preceding and others of a similar character as showing the high value placed upon this substance by peoples that had hardly made a start along the highway of civilization. With respect to the above-mentioned method of making an impure grade of salt, it is worth noting that it is also spoken of as employed elsewhere. Varro had heard of a region where the inhabitants knew no salt, but used instead as seasoning a kind of salt coals which they obtained from burning wood. The same method and the same substitute for real salt are also reported as employed by some of the natives of Spain. Pliny devotes a good deal of space in his 'Natural History,' that storehouse of information and imagination, to the consideration of salt. He enumerates somewhat in detail the different places in almost the entire known world where it is found, describes the various methods of its production, notes the fondness of cattle for it, and adds that when mixed with their food it increases the quantity and improves the quality of the cheese. According to him Ancus Martius, the fourth king of Rome, established the first salt works, and the Romans perform no sacred rites without *mola salsa*. By the Romans salt was regarded as almost the staff of life, and the salt-cellar was preserved in families because it was supposed to have a quasi-sacred character. In one of his Odes, Horace tells his friend, Grosphus, that the man who enjoys life is he whose father's salt-cellar gleams on his table. In a satire by the same poet, the rustic sage informs the epicure that bread with salt will

appease his growling stomach, and advises him to spurn dainty viands. The cognomen Salinator, borne by a member of the Livian gens, came into prominence for two reasons. The first who received the appellation is said to have imposed a new impost on salt. He is further distinguished for the magnanimity he displayed in laying aside his private grudge against the other consul, Claudius Nero, for the good of the commonwealth. The hearty cooperation of the two commanders-in-chief and their armies led to the death of Hasdrubal and the complete destruction of his army. Wherever a system of taxation is framed with a view to raising the largest possible revenue, the heaviest burden falls on the necessaries of life. From almost time immemorial salt has had to bear a disproportionate share of this load. It is probable that in ancient times all regularly organized governments derived some revenue from this commodity. In Italy, as we have seen, the beginning was made long ago, though the details are lacking. In that country it is still a government monopoly. The profits realized are about thirteen hundred per cent., and its cost is almost prohibitive to the very poor. Such a delicacy do their children consider it that if they are allowed to choose between sweetmeats and salt they take the latter in preference. That a more liberal use of salt would improve the health and sanitary condition of this class hardly admits of a doubt.

It is safe to say that no article of consumption has been so ruthlessly exploited by governments to the detriment of their subjects as this one. Taking advantage of the fact that it is a necessary concomitant of the food of man and beast, they have made it an important source of revenue because its payment could not be evaded. In France under the ancient régime the tax on this article differed a good deal in the different provinces, but its transportation from one into another was prohibited. Its manufacture was also limited, and that which was produced by natural evaporation on the coasts was thrown back into the sea by the fiscal agents. While the price was enormous, the great majority of the citizens were not allowed to buy as small a quantity as they chose; they were compelled to pay for a certain amount conditioned upon the size of the family. On the other hand, certain privileged persons received all the salt they wanted gratis; or, if they preferred, they had the prerogative of receiving money in lieu thereof. The king did not directly control the salt monopoly. He acted through an association of revenue farmers who paid into the fisc a fixed sum, after which they had the legal right to exploit their helpless victims to the utmost. They possessed police powers and used them unmercifully. Evasions of the salt laws were rigorously punished by the judges, who were almost always hand in glove with the salt-farmers. Every year for nearly two centuries there were from two to three thousand arrests. Those who were found guilty were

subjected to fines, to the lash and to the galleys. In case of a second conviction they were sometimes hanged. The peasant was prohibited from using salt a second time. The brine from meat or fish had to be thrown away; it could not be used in the kitchen or taken to the stables for the cattle. It was illegal for any one to make salt from sea-water even for his own use, and equally illegal to water animals with natural brine. To prevent tanners and leather-dressers, who employed salt in their industries, from putting it to any other use the salt farmers often poisoned it. Owing to the large number of different governments in Germany and owing to some divergencies in matters of internal administration, one can not make a statement on this point that is applicable to the entire country. But in view of the strong inclination of many of the German monarchs to ape French customs, especially the bad ones, it is safe to say that the salt monopoly in the empire was quite as oppressive as in France. It may be added that on the whole the French peasant was not as badly treated as his German brother; the former first shook off much of the burden by drastic means from causes that need not be considered here. So late as 1840 a sort of salt conscription was enforced in Saxony which required each family to buy a certain quantity of it and prohibited its sale to a second party. In Prussia a similar regulation was abolished in 1816. Salt was a government monopoly in the greater part of Germany until 1867, as it still is in Austria, Italy and some other countries. In Austria all salt works belong to the government; such was also the case in some other south German states until recently. It likewise owns all salt-yielding territory. At present there is a general revenue law for the empire and a duty on the foreign product. It is therefore a good deal cheaper in the German than in the Austrian empire. While it is doubtful whether any article of consumption has so long afforded governments a means of oppressing their subjects as salt, and while its history makes an interesting though rather gruesome chapter in political economy, it is, nevertheless, unfair to judge the ruling powers of the past by contemporary standards. Until comparatively recent times economic laws were so little understood and rulers were always so hard pressed for money that they were constrained to resort to such measures for raising revenue as promised the largest and most certain returns. In the nature of the case a commodity in such demand as salt had to bear a disproportionate share of the public burdens. Cruel and inhuman methods of legal procedure were the order of the day, and those who suffered from it did not themselves know any better way of attaining the ends in view. It is greatly to the credit of the English people that their jury system did much to mitigate the penalties to which many a transgressor against the revenue laws as against other laws made himself liable. Though juries could not change the statutes, they

refused to convict when the penalty seemed too great for the offense.

The United States has never collected revenue from salt, but when provision was made by congress for the government of the Northwest Territory and for the sale of lands therein, it took care to reserve the salt licks, apparently fearing that they might be made a means of extortion to the consumers of this indispensable article of diet. One section of the act of congress reads:

> That a salt spring lying upon a creek which empties into the Scioto River, on the east side, together with as many contiguous sections as shall be equal to one township, and every other salt spring which may be discovered, together with the section of one mile square, which includes it, also four sections at the center of every township, containing each one mile square, that shall be reserved for the future disposal of the United States; but there shall be no reservation except for salt springs, in fractional townships, where the fraction is less than three fourths of a township.

We read of bloody battles between Germanic tribes for the possession of salt springs, and the inference is perfectly fair that the rumor of very few has come down to us by means of the written and the printed page. In the new world rival Indian tribes in like manner often contended fiercely for the same flowing treasure. Here too we find a repetition of the nomenclature of primitive Europe. There are several salt rivers in the states formed out of the Northwest Territory besides salt creeks, salt licks and other names, due to the presence of natural salt. The number is doubtless very much larger than the list given in the ordinary gazetteers, as the insignificant ones are not mentioned.

Although there are few regions in any part of the world in which there are neither saline springs nor deposits of rock-salt, it is probable that the Aryan name was derived from the sea and that the first salt was obtained from it by natural evaporation. In Homer *áls* means both salt and the sea; or perhaps it would be better to say that salt is named from the sea because the saline property of sea-water is its most salient characteristic. The designation *áls* is more particularly applied to that part of the sea which is near the land, as also to its bays and inlets, those parts with which man in the nature of the case was most familiar. In the Roman territory there existed in ancient times a Via Salaria, or Salt Road, which extended from the territory of the Sabines to the mouth of the Tiber, along which these people were permitted to transport salt for domestic use from the Mediterranean through the Roman country. The early Italians were, therefore, also dependent on the sea for their salt. It is noteworthy that Homer does not mention salt as employed in connection with sacrificial ceremonies. On the other hand, Virgil speaks of it as in regular use among the Romans, as do also other writers. While it is always

unsafe to base conclusions on the evidence of silence, another ancient author quoted by Athenaeus says that in former times the Greeks burned the sacrificial parts of animals without salt, and that the custom continued into later times in conformity with the ancient practise. Here then we have Homer's silence supplemented by positive testimony. It is well known, moreover, that all peoples are more conservative in religious usages than in any other. The adhibition of salt, the *mola salsa* of the Romans, seems not to have been borrowed from the Greeks, as were so many of their religious ceremonies. Like the Romans with their salted meal, the Hebrews were careful not to omit salt from their sacrifices, though the former may not regularly have put it on the flesh of the slain victims. In Leviticus we read: "And every oblation of thy meal offering thou shalt season with salt, neither shalt thou suffer the salt of the covenant of thy God to be lacking from thy meal offering: with all thine oblations thou shalt offer salt." From this command it may be inferred that salt was a part of bloody sacrifices as well as of those of the fruits of the earth.

In Germany there are many place-names that contain the Keltic root *hal* which seem in some way to be connected with sodium chloride. The best known of these is the city of Halle on the river bearing the Teutonic appellation, Saale. It is not easy to see how this double designation originated and conjectures are feeble arguments. There is no doubt, however, that Halle got its name from the salt springs near it. In the same country there were anciently several rivers called Sala on the banks of which salt works bearing the name Hall were planted. Besides the Halle already mentioned there is Reichenhall in Bavaria, Hallein in Salzburg, Hall in Tyrol and in Swabia, as also Halen in Brabant, and others. In Czech there are likewise a number of words containing the radical *hal* that have some connection with salt. This root is still distinctly preserved in the Welsh 'halen' salt. In some of the Keltic dialects, however, the initial h is represented by s.

In England there are a number of inland towns to the names of which the suffix wich, from the Norse *wic,* a bay, is appended. This seeming absurdity is easily understood when we remember that a wych-house or wickhouse and a bayhouse came to be regarded as synonymous terms, and that wychhouses were erected where salt was prepared from brine, though they might be far from a bay. In the same way a coarse kind of salt came to be called baysalt from its similarity to the crude article of primitive manufacture. The wics in Essex were probably the first localities where salt works of the rude original type were erected. According to Isaac Taylor, the Domesday Book gives the names of three hundred and eighty-five places in Sussex alone where salt was made. The number seems incredible and may be a misprint; but the general fact is well established.

In Great Britain, as on the Continent, salt was obtained before the advent of the Teutons or the Romans. Here, too, we find our guide in the syllable *'hal,'* which occurs in place-names in Carnarvon, in Hampshire, in Lancaster and elsewhere. Plutarch has left upon record some evidence that points to a period when salt was practically unknown in Egypt. He says the priests will permit no salt upon their tables, will not address a pilot because of his occupation at sea, and that they also eschew fish for the same reason. Another passage seems to modify this strong statement to this extent that there are certain times when the priests do not partake of salt for the reason that it increases the desire for food and drink. All Greek evidence on such points is, however, of small value, since to the Greeks Egypt was at all times a wonderland where the most singular and unique customs prevailed. Long before Plutarch's time Herodotus reported to his countrymen that the people of the Nile valley did everything different from his own countrymen. A special ceremony or a custom observed only on particular occasions was easily perverted to a general usage by persons who had merely a superficial knowledge of the conditions.

Northern Africa has from time immemorial been a great storehouse of salt. Thebes in Egypt was the starting point for caravans that moved across it towards the west, perhaps as far as the Niger. Herodotus relates that a ten-day journey from the city heaps of the mineral lie in large lumps upon the hills and that from the tops of these hills salt water gushes forth. It is in this region that the Ammonians dwell, in whose district is the celebrated temple of Jupiter Ammon. The oasis is the bottom of what was once a salt lake or part of the sea and still has many salt springs in it. The soil is also impregnated with salt, although there is no scarcity of fresh water. It is probable that the chemical compound known as sal ammoniac gets its name from this region, either because it was first manufactured here or because it was found here in its natural state.

In many parts of northern Africa, often at long distances from the coast, salt occurs in great abundance. Though there is generally stone in plenty, the inhabitants in some places use blocks of salt for constructing dwellings, since it is easier handled and there is no danger to be apprehended from rain, which rarely falls in this part of the world. The salt blocks employed for this purpose are, however, not pure. They are cemented with mud, probably owing to a scarcity of lime. Some portions of the Sahara are covered with a crust of salt to such an extent as to give long reaches the appearance of being covered with a recent fall of snow. Some of the statements of Herodotus and other ancient writers are perhaps exaggerated, but many of them are corroborated by recent explorers. M. Dubois in his work, 'Timbuctoo the Mysterious,' affirms that salt is as highly valued as ever in

this part of the world, in spite of its great abundance. He found salt mines in the heart of the desert near a place called Thegazza. For the Soudanese salt has from time immemorial represented, and still represents, the principal article of commerce and their most precious commodity. The long depression in the western Sahara bearing the name of El Djouf is a vast mine of rock salt. The salt mines of Thegazza were abandoned in the sixteenth century for those of Taoudemi, nearer Timbuctoo. The same explorer reports that even here the houses are built of rock salt and roofed with camel skins. Under a thin covering of sand the mineral is found in clearly marked layers. It is dug out in large lumps and trimmed down to blocks about three and a half feet long by one and one fourth feet in breadth. It looks like bars of red or gray-veined marble, and as they come out of the mine they are stamped with the trade-mark of the different contractors. At Timbuctoo they are embellished with designs in black paint and the name of some venerated chief is written on them in Arabic characters. They are then bound round with thongs of raw leather so arranged as to hold the parts together in case of fracture. The densest and whitest blocks are most in demand, those veined with red being of an inferior quality. Timbuctoo is the entrepot of the whole region lying southeast as far as Lake Chad. There is nothing that the Soudanese possesses that he refuses to part with for a lump of salt. To these people it is more valuable than gold itself.

In ancient as well as modern times the partaking of salt with another person was regarded as the symbol of friendship and hospitality. Among the Slavic peoples it is still the custom to welcome the stranger with a proffered gift of salt and bread; while in cases of dispute the Arab is wont to appeal to the bread and salt he has eaten with his adversary as proof of sincerity. The advice embodied in the injunction, 'Before you make a friend, eat a bushel of salt with him,' has been proverbial from the remotest times. Both Aristotle and Cicero refer to it as current in their time. An ancient commentator on Homer says that salt is regarded as the symbol of friendship, *par excellence,* either because it was offered to guests before anything else, or because salt more than any other substance is a prophylactic against decay. In Numbers certain offerings are enumerated as constituting 'a covenant of salt for ever before the Lord unto thee and thy seed with thee.' Perhaps the custom of handing down the salt vessel from generation to generation in Roman families has some connection with the idea of incorruption.

The word *salt* has impressed itself on our language in a curious way in our term 'salary.' So necessary did the Romans consider salt to the efficiency of their armies that each soldier was provided with a special ration of it, or with the means of providing it. This stipend

was called *salarium argentum*. Civil officials or military officers when traveling in a civil capacity were also provided with this ration of salt. In later times, when the commodity was no longer difficult to obtain, money was paid in lieu of salt, but still ostensibly for the purpose of providing the same article. Generally, however, the allowance was sufficiently liberal to purchase a good many things besides sodium chloride. In time salt-money in ancient Rome came to be as comprehensive as 'stationery' in the phraseology of our home-grown legislators. The officials received no salary, yet the unfortunate provincials would generally have been glad to pay a definite amount rather than the presents (?) and perquisites which they were called upon to provide. A salary usually means a fixed sum, but there never has been framed a clear definition of 'necessary expenses.'

As indicated above, it is still a mooted question whether the consumption of salt is essential to the maintenance of animal life. If, as is now generally held, marine fauna antedated all others, it is reasonable to suppose that the principle of atavism would never carry living beings beyond a natural fondness for and even the necessity of consuming saline matter. On the other hand, it is maintained by some competent authorities that a sufficient quantity is taken into the system by the herbivora to supply all natural requirements. From these it passes into the bodies of the carnivora. Those who insist that sufficient salt is taken into the animal body indirectly with the food are equally positive that the excessive fondness for it exhibited by most men and some other animals is the result of a perverted taste. They cite as a parallel case the eagerness with which dogs and other brutes, to say nothing of human beings, devour sweetmeats, as evidence of a vitiated taste that readily results in more or less serious harm. Certain it is that no mineral substance has ever been so eagerly sought as an ingredient of food and it is probable that the quantity consumed is on the increase. But whether animal life is possible under conditions where salt is wholly absent can, in the present state of our knowledge, be neither categorically affirmed nor positively denied.

# WALTER REED.*

By MAJOR WALTER D. McCAW,
SURGEON U. S. ARMY.

IT is given to but few scientific men to lay bare a secret of nature materially affecting the prosperity of nations, and the lives, fortunes and happiness of thousands. Fewer still succeed in so quickly convincing brother scientists and men in authority of the truth of their discoveries that their own eyes behold the glorious result of their labor. Of the fifty-one years of Walter Reed's industrious, blameless life, twelve only were spent in the study of the special branch of science in which he became famous, but his name now stands with those of Jenner, Lister and Morton, as among the benefactors of humanity.

Walter Reed was born in Gloucester County, Virginia, September 13, 1851, the son of the Rev. Lemuel Sutton Reed and Pharaba White, his wife. The circumstances of his family were modest, and some of the years of his boyhood were spent in a much troubled section of the south during the great civil war. He acquired, however, a good preliminary education, and at an age when most boys are still in the schoolroom, he began the study of medicine at the University of Virginia, graduating as M.D. in 1868, when only seventeen years old. A second medical degree was received later from Bellevue Medical College, New York, and then came terms of service in the Brooklyn City Hospital, and the City Hospital, Blackwell's Island. Before the age of twenty-one, Reed was a district physician in New York City, and at twenty-two one of the five inspectors of the Board of Health of Brooklyn.

He entered the army of the United States as assistant surgeon with the rank of first lieutenant, in 1875, and for the next eighteen years, with the usual varying fortunes of a young medical officer of the army, he served in Arizona, Nebraska, Dakota and in the southern and eastern states. According to the exigencies of the service he was moved frequently from station to station, everywhere recognized by men of his own age as a charming and sympathetic companion, and by older officers as an earnest and intelligent physician, whose industry, fidelity to duty, and singularly clear judgment, gave brilliant promise for the future. In the poor cabins and dugouts of the pioneers in the sparsely settled districts where he served his flag, Reed was ever a messenger of healing and comfort. At that time army posts on the frontier were

* A memoir, published by the Walter Reed Memorial Association. Contributions to the memorial fund may be sent to the treasurer, Mr. Chas J. Bell, President American Security and Trust Co., Washington, D. C.

usually remote and with small garrisons. The young medical officer, generally the only one at the station, was called upon by the settlers for miles around. Without help, and with only such instruments and medicines as could be hastily stuffed in his saddle-bag, he was summoned to attend a fractured thigh, a child choking with diphtheria, or, most trying of all, a complicated child-birth.

Such experience schools well in self-reliance, and in the formation of quick and accurate observation. For a man like Reed, already an earnest student, no better preparation could perhaps have been had. His earlier army service must have singularly tended to develop in him the very qualities most necessary to his final success. To the end of his life it was noticeable that even when he had long given up the practice of medicine for the work of the laboratory, he was, nevertheless, unexcelled at the bedside for rapid unerring diagnosis and sound judgment in treatment. So also were the series of experiments which robbed yellow fever of its terrors especially remarkable for simplicity, accuracy and completeness, or they never would have so quickly convinced the world of their truth. Too much reverence for accepted teachings, and too little experience in grappling with difficulties unassisted, and they might never have been conceived or carried out.

In 1890, he was assigned to duty in Baltimore and remained there over a year. Here he had the great advantage of working in the laboratories of Johns Hopkins University and the happiness of winning the close friendship of his distinguished teacher, Professor William H. Welch.

In 1893 Reed was promoted surgeon with the rank of major, and in the same year was detailed in Washington as curator of the Army Medical Museum and professor of bacteriology at the newly organized Army Medical School. Here he worked industriously at his specialty and wrote many valuable monographs, all characterized by accuracy and originality. His excellent judgment made him especially valuable in investigating the causes of epidemic diseases at military posts and in making sanitary inspections. He was, therefore, frequently selected for such work, which, with his duties as teacher and member of examining boards, occupied much of the time that he would otherwise have spent in his laboratory. Here, again, it seems that duties which must often have been irksome were specially fitting him for his culminating work.

During the Spanish-American war the camps of the volunteer troops in the United States were devastated by typhoid fever, and Major Reed was selected as the head of a board to study the causation and spread of the disease. This immense task occupied more than a year's time. With the utmost patience and accuracy the details of hundreds of individual cases were grouped and studied. The report of the commission, now in course of publication by the government, is a monumental work which must always serve as a basis for future study

of the epidemiology of typhoid fever. The most original and valuable work of the board is the proof that the infection of typhoid fever is spread in camps by the common fly, and by contact with patients and infected articles, clothing, tentage and utensils, as well as by contaminated drinking water.

In June, 1900, Major Reed was sent to Cuba as president of a board to study the infectious diseases of the country, but more especially yellow fever. Associated with him were Acting Assistant Surgeons James Carroll, Jesse W. Lazear and A. Agramonte. At this time the American authorities in Cuba had for a year and a half endeavored to diminish the disease and mortality of the Cuban towns, by general sanitary work, but while the health of the population showed distinct improvement and the mortality had greatly diminished, yellow fever apparently had been entirely unaffected by these measures. In fact, owing to the large number of non-immune foreigners, the disease was more frequent than usual in Havana and in Quemados near the camp of American troops, and many valuable lives of American officers and soldiers had been lost.

Reed was convinced from the first that general sanitary measures alone would not check the disease, but that its transmission was probably due to an insect. The fact that malarial fever, caused by an animal parasite in the blood, is transmitted from man to man through the agency of certain mosquitoes had been recently accepted by the scientific world; also several years before, Dr. Carlos Finlay, of Havana, had advanced the theory that a mosquito conveyed the unknown cause of yellow fever, but did not succeed in demonstrating the truth of his theory.

Dr. H. R. Carter, of the Marine Hospital Service, had written a paper showing that although the period of incubation of yellow fever was only five days, yet a house to which a patient was carried did not become infected for from fifteen to twenty days. To Reed's mind this indicated that the unknown infective agent has to undergo a period of incubation of from ten to fifteen days, and probably in the body of a biting insect. Up to this time the most generally accepted theory as to the causation of yellow fever was that of Sanarelli, who claimed that the *Bacillus icteroides* discovered by him was the specific agent of the disease. Major Reed, in association with Dr. Carroll, had, however, already demonstrated that this bacillus was one widely disseminated in the United States, and bore no special relation to yellow fever.

In June, July and August, 1900, the commission gave their entire attention to the bacteriological study of the blood of yellow fever patients, and the post-mortem examination of the organs of those dying with the disease. In twenty-four cases where the blood was repeatedly examined, as well as in eleven carefully studied autopsies, *Bacillus icteroides* was not discovered, nor was there any indication of the presence in the blood of a specific cause of the disease.

Application was made to General Leonard Wood, the military governor of Cuba, for permission to conduct experiments on non-immune persons, and a liberal sum of money requested for the purpose of rewarding volunteers who would submit themselves to experiment. It was, indeed, fortunate that the military governor of Cuba was a man who by his breadth of mind and special scientific training could readily appreciate the arguments of Major Reed as to the value of the proposed work. Money and full authority to proceed were promptly granted, and to the everlasting glory of the American soldier, volunteers from the army offered themselves for experiment in plenty, and with the utmost fearlessness.

Before the arrangements were entirely completed, Dr. Carroll, a member of the commission, allowed himself to be bitten by a mosquito that twelve days previously had filled itself with the blood of a yellow fever patient. He suffered from a very severe attack, and his was the first experimental case. Dr. Lazear also experimented on himself at the same time, but was not infected. Some days later, while in the yellow fever ward, he was bitten by a mosquito and noted the fact carefully. He acquired the disease in its most terrible form and died a martyr to science, and a true hero. No other fatality occurred among the brave men who, in the course of the experiments, willingly exposed themselves to the infection of the dreaded disease.

A camp was especially constructed for the experiments about four miles from Havana, christened Camp Lazear in honor of the dead comrade. The inmates of the camp were put into most rigid quarantine and ample time was allowed to eliminate any possibility of the disease being brought in from Havana. The personnel consisted of three nurses and nine non-immunes, all in the military service, and included two physicians.

From time to time Spanish immigrants, newly arrived, were brought in directly from the immigrant station; a person not known to be immune was not allowed to leave camp, or if he did, was forbidded to return. The most complete record was kept of the health of every man to be experimented upon, thus eliminating the possibility of any other disease than yellow fever complicating the case.

The mosquitoes used were specially bred from the eggs and kept in a building screened by wire netting. When an insect was wanted for an experiment it was taken into a yellow fever hospital and allowed to fill itself with the blood of a patient; afterward at varying intervals from the time of this meal of blood it was purposely applied to non-immunes in camp. In December five cases of the disease were developed as the result of such applications; in January, three, and in February, two, making in all ten, exclusive of the cases of Drs. Carroll and Lazear. Immediately upon the appearance of the first recognized symptoms of the disease, in any one of these experimental cases, the patient was taken from Camp Lazear to a yellow fever hospital, one

mile distant. Every person in camp was rigidly protected from accidental mosquito bites, and not in a single instance did yellow fever develop in the camp, except at the will of the experimenters. The experiments were conducted at a season when there was the least chance of naturally acquiring the disease, and the mosquitoes used were kept active by maintaining them at a summer temperature.

A completely mosquito-proof building was divided into two compartments by a wire screen partition; infected insects were liberated on one side only. A brave non-immune entered and remained long enough to allow himself to be bitten several times. He was attacked by yellow fever, while two susceptible men in the other compartment did not acquire the disease, although sleeping there thirteen nights. This demonstrates in the simplest and most certain manner that the infectiousness of the building was due only to the presence of the insects.

Every attempt was made to infect individuals by means of bedding, clothes and other articles that had been used and soiled by patients suffering with virulent yellow fever. Volunteers slept in the room with and handled the most filthy articles for twenty nights, but not a symptom of yellow fever was noted among them, nor was their health in the slightest degree affected. Nevertheless, they were not immune to the disease, for some of them were afterward purposely infected by mosquito bites. This experiment indicates at once the uselessness of destroying valuable property for fear of infection. Had the people of the United States known this one fact a hundred years ago, an enormous amount of money would have been saved to householders.

Besides the experimental cases caused by mosquito bite, four non-immunes were infected by injecting blood drawn directly from the veins of yellow fever patients in the first two days of the disease, thus demonstrating the presence of an infectious agent in the blood at this early period of the attack. Even the blood serum of a patient, passed through a bacteria-proof filter, was found to be capable of causing yellow fever in another person.

The details of the experiments are most interesting, but it must here suffice to briefly sum up the principal conclusions of this admirable board of investigators of which Reed was the master mind:

1. The specific agent in the causation of yellow fever exists in the blood of a patient for the first three days of his attack, after which time he ceases to be a menace to the health of others.

2. A mosquito of a single species, *Stegomyia fasciata,* ingesting the blood of a patient during this infective period is powerless to convey the disease to another person by its bite until about twelve days have elapsed, but can do so thereafter for an indefinite period, probably during the remainder of its life.

3. The disease can not in nature be spread in any other way than

by the bite of the previously infected *Stegomyia.* Articles used and soiled by patients do not carry infection.

These conclusions pointed so clearly to the practical method of exterminating the disease that they were at once accepted by the sanitary authorities in Cuba, and put to the test in Havana, where for nearly a cenury and a half, by actual record, the disease had never failed to appear annually. In February, 1901, the chief sanitary officer in Havana, Major W. C. Gorgas, Medical Department, U. S. Army, instituted measures to eradicate the disease, based entirely on the conclusions of the commission. Cases of yellow fever were required to be reported as promptly as possible, the patient was at first rigidly isolated, and immediately upon the report a force of men from the sanitary department visited the house. All the rooms of the building and of the neighboring houses were sealed and fumigated to destroy the mosquitoes present. Window and door screens were put up, and after the death or recovery of the patient, his room was fumigated and every mosquito destroyed. A war of extermination was also waged against mosquitoes in general, and an energetic effort was made to diminish the number bred by draining standing water, screening tanks and vessels, using petroleum on water that could not be drained, and in the most systematic manner destroying the breeding places of the insects.

When the warm season returned a few cases occurred, but by September, 1901, the last case of yellow fever originated in Havana, since which time the city has been entirely exempt from the terrible disease, that had there kept stronghold for a hundred and fifty years. Cases are now admitted into Havana from Mexican ports, but are treated under screens with perfect impunity, in the ordinary city hospitals. The crusade against the insects also caused a very large decrease in malarial fevers.

The destruction of the most fatal epidemic disease of the western hemisphere, in its favorite home city is but the beginning of the benefit to mankind that may be expected to follow the work of Reed and his associates. There can be no manner of doubt should Mexico, Brazil and the Central American Republics, where the disease still exists, follow strictly the example set by Havana, that yellow fever will become extinct and the United States forever freed from the scourge, that has in the past slain thousands of our citizens and caused the loss of untold treasure.

More recent investigations into the cause and spread of yellow fever have only succeeded in verifying the work of Reed and his commission in every particular and in adding very little to our knowledge of the disease. Later researches by Guiteras in Havana, by the Public Health and Marine and Hospital Service in Vera Cruz, and lastly by a delegation from the Pasteur Institute of Paris in Rio de Janeiro, all confirm in the most convincing manner, both the accuracy and comprehensive-

ness of the conclusions of the American commission. It has been well said that Reed's experiments 'will always remain as models in the annals of scientific research, both for the exactness with which they were adapted to the points to be proved and the precautions taken that no experiment should be vitiated by failure to exclude all possible sources of error.'

Appreciation of Reed's work was instant in the scientific world. Honorary degrees from Harvard University and the University of Michigan were conferred upon him, learned societies and distinguished men delighted to honor him, and after his death congress voted a special pension to his widow.

To the United States the value of his services can not be estimated. Ninety times has yellow fever invaded the country, carrying death and destruction, leaving poverty and grief. New Orleans, Memphis, Charleston, Galveston, Portsmouth, Baltimore, Philadelphia, New York and many smaller towns have been swept by the disease. The epidemic of 1853 cost New Orleans eight thousand lives, that of 1793 wiped out ten per cent. of Philadelphia's population. The financial loss to the United States in the one epidemic of 1878 was estimated as amounting to fifteen million three hundred and thirty-five thousand dollars; but suffering, panic, fear and the tears of widows and orphans can never be estimated. Now, however, if yellow fever should again cross our southern border, there need be no disturbance of commerce or loss of property in the slightest degree comparable with that which epidemics in the past have caused.

The death of Major Reed took place November 23, 1902, in Washington, from appendicitis. It is gratifying to think that, although his country and the scientific world were deprived of one from whose future services more benefit to humanity might reasonably be expected, nevertheless he was privileged before his life's close to know that his discovery had been tested, and that a great city was freed from her ancient foe, to know that his conscientious work had contributed immeasurably toward the future prospects of an infant republic, and even more to the welfare of his own beloved country, whose flag he had served so faithfully.

In the national capital and in the great cities of the United States there are stately monuments to the country's great ones. Statues of warriors, statesmen and patriots stand as silent witnesses of a people's gratitude. Is there not room for the effigy of Walter Reed, who so clearly pointed out to his fellow man the way to conquer America's worst plague?

# THE ROYAL PRUSSIAN ACADEMY OF SCIENCE AND THE FINE ARTS. BERLIN.

By EDWARD F. WILLIAMS,

Chicago, Ill.

## *VI. The History of the Academy under the Emperor William I., the Emperor Frederick III. and his son William II., the present Emperor, or from 1859 to 1900.*

As early as 1860 A. Kirchhoff, in an address delivered on one of the festival days of the academy, emphasized the change which had been introduced into the methods of scientific study. Research, he said, had limited itself to narrow fields with a view to the mastery of the least important detail in them. This limitation he regarded as necessary. Although the academy had done its part in the discovery and confirmation of the law of the conservation of energy, and had shown the immense value of the law of evolution as a scientific hypothesis, the time had come when investigation must be content to confine itself to a limited field, if its results are to be trustworthy, and permit men of comprehensive minds and more general information to weave them into consistent philosophical systems. The era of the universal had passed, that of the particular had begun.

The Prince of Prussia, brother of the king, became regent in 1859 and king in 1860. He was succeeded by his son, Frederick III., who, after a reign of three months, was followed by his son William II., who is still on the throne. Each of these sovereigns has favored the academy so far as possible.

From 1859 to 1900, 82 members were received into the academy. Of the 46 actively engaged in its work in 1859, Rammelsberg and Mommsen alone were living in 1900. Of the 82 new members, 32 had died and 4 had moved from Berlin. The physical class had lost but 11 members, the historical 25. Of some of these members a few words may be permitted. Helmholtz, the discoverer of the law of the conservation of energy, has been thought in Germany worthy of a place by the side of Sir Isaac Newton. His works on optics, acoustics and the physiology of the nerves are known everywhere and are received as authority. Von Siemens is famous for his discoveries in electricity and the practical use he made of them. Virchow, and van't Hoff the chemist, still living, have brought the academy lasting fame. During

the period from 1860 to 1900 the names of nine eminent historians were on its books. Droysen, Duncker, Waitz, von Sybel, Wattenbach, Nitzsch, Weitszaecker, Lehmann, von Tretschke, the last named representing the school of Ranke, one of the most illustrious of the eminent historians Germany has produced. Olshausen and Roediger represented the Hebrew language and literature as well as that of Syria and Arabia, and Dillmann, in addition to a thorough knowledge of Hebrew and Arabic, was especially famous for his mastery of Ethiopic.

Prior to 1870 the income of the academy had been very inadequate, although private gifts from the king and grants from the government had enabled it to carry out to a successful conclusion several important enterprises. For scientific purposes it had never received from its own resources more than $2,300 in any single year. By a cabinet order, dated at Versailles, March 2, 1871, the Archeological Institute of Rome, through which so much has been accomplished, was made a state institution, brought into connection with the academy and put under its control. On May 16, 1874, its name was changed to that of The Imperial German Institute of Archeology, and a branch of it established in Athens for the study of Grecian antiquities. In 1875 an agreement was made with the academies in Vienna and Munich whereby each of them shares in the expense and direction of this archeological work. Through the generosity of the government in 1874 the academy was in a position to expend for science three times as much as formerly. The figures show that between 1877 and 1897 nearly $350,000 were set aside for scientific purposes alone. During the reign of William I. institutes were organized and equipped in connection with all the Prussian universities, and most of the other German universities, for the training, under the best skill at command, of young men for research in special fields of scientific study. In these institutes some of the most striking of recent discoveries have been made.

The academy made its interest in astronomy manifest in the part it had in expeditions for the study of the occultations of Venus, in magnetism and meteorology by two expeditions sent to the poles for the study of these branches of science. The king had long been interested in the geodetic institute and in archeological studies, in the society formed for the publication of the Dutch sources of history, and shortly before his death he had determined to aid the work which had been begun on the Monumenta Borussia. He had opened the archives of the state to historical students from every part of the world. He had taken a deep interest in the excavation at Olympia and Pergamon, through the results of which the academy and the nation acquired well-deserved fame.

While the academy had every reason to look for sympathy and assistance from the Emperor Frederick III. and his wife, an appeal

to his son, after the father's untimely death, was not in vain. Means for aid in making a new dictionary of the Latin language on the most extensive scale possible, and for other important enterprises, were asked for and granted. Toward the cost of the dictionary the academies in Munich, Göttingen, Leipzig and Vienna contribute and share in directing and furnishing the labor which must be done on it. It is estimated that this will extend over twenty years at least, and require the aid of a score of men. The headquarters of the work are at Munich. An edition of the works of Kant worthy his name has been published, another of the writings of William von Humboldt, another of the mathematical works of Weierstrass. A dictionary of the old Egyptian language has been planned, and work in gathering material for it, in which scholars from different countries are taking part, is now progressing. The value of the academy as a mediator in projecting and carrying out costly works is illustrated in the excavations at Olympia and Pergamon. For the former the sum of $75,000 was granted. The work was planned solely in the interests of scholarship, with the agreement that all articles of value discovered by the excavations should be the property of Greece and be left within its limits. Suggested by Professor Curtius, entrusted to the care of a man of his selection, the first spadeful of earth was turned on October 4, 1875, and six years later the last. The results astonished the learned world, and not less so those obtained at Pergamon.

Preparations for observing the occultations of Venus were begun by Minister von Muhler as early as 1869, and an expedition, at the cost of the academy, was sent to Luxor in 1874, and another to Punta Arenas in 1883. Both were under the direction of the astronomer Auwers, who has published the results of his discoveries in six volumes. In 1878 plans were laid for a proper celebration of the four hundredth anniversary of Luther's birth, and the academy, in carrying out the wishes of the king and of the nation, offered a prize for a perfect edition of the writings of the reformer prior to 1521. The prize was awarded to E. Henrici in 1880, and in June, 1883, arrangements were made for a complete and standard edition of all Luther's works.

The year 1874 is remembered by the academy as the year in which its income was so increased as to enable it to undertake enterprises previously beyond its reach and at the same time to assist individuals in work for which their private means were insufficient. This change in its affairs was happily emphasized by Mommsen in his speech in July, 1874. In it he said there is something more in the world than Latin and Greek, than the upheaval of mountains or than the counting of figures, important as the academy deems them. The academy is and must be a common meeting place for all men of science, and must show an interest in whatever men of science of any nationality may

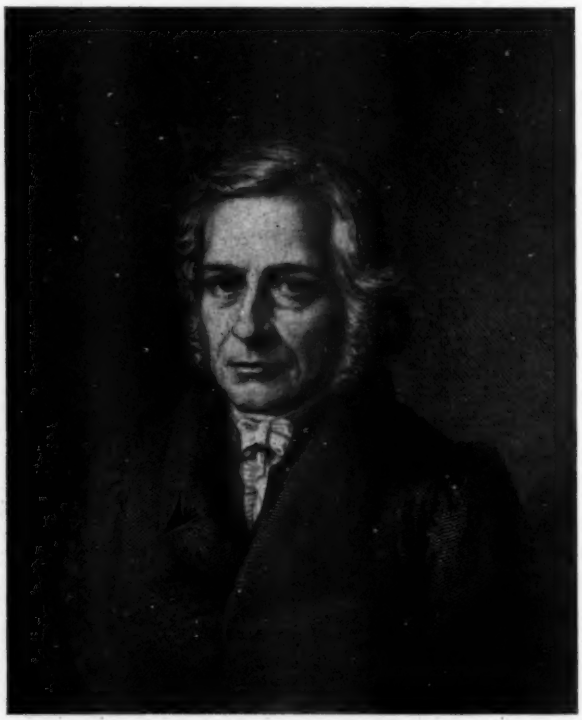
do, and thus put an end to everything like narrowness or selfishness in one's own work. The academy, as has already been seen, had begun to act, as it has continued to do, as a mediator between the government, which has the funds for important enterprises, and the men who, though poor, have the ability successfully to carry them out. Hence it is that for a quarter of a century at least the academy has been able to direct most of the great scientific enterprises of Germany and has given impulse and needed assistance to private efforts in narrow and limited, yet important fields of research. The income, which increases nearly every year with gifts by will and from people interested in its work, in 1900 amounted to 213,462 Marks, a little more than $53,000. The income had averaged from 1897 to 1900 136,462 Marks, or a little more than $39,000. Since May, 1898, one third of the interest of the Frau Maria Elizabeth Wentzel-Heckmann foundation, or of a capital of 1,500,000 Marks, has been available for scientific enterprises of the first magnitude. At the death of Frau Heckmann the interest of the entire sum will be available for the spread of scientific knowledge. It is stipulated that the income shall not be limited to a single field. While the academy may suggest the field to which the money shall be given, its final disposal is in the hands of a commission composed of the cultus minister and six persons, three of whom are to be chosen by the academy every five years. Thus far the gift has furnished means for a dictionary of the German law language, justified the academy in beginning the publication of an edition of the oldest Greek writers, which will embrace not less than fifty volumes, and provided for the equipment of an expedition to German East Africa for the study of natural history. A good deal of money is expended every year for prizes, although these are less favored than formerly, and an increasing amount in aiding individuals in special work of importance.

Changes in some of the statutes of the academy were adopted on March 2, 1881, by which its efficiency has been very much increased. The number of general meetings was reduced one half and those of the classes and their sections increased one half. The number of active members was put at 54, 27 for each class. The foreign members were reduced from 16 to 10 for each class, and since 1882 reports, formerly published every month, are now published weekly. These reports are of the highest value and are indispensable to those who would keep abreast of the advance made by Germany in scientific, historical or philosophical studies. Active members are paid 900 Marks ($225) a year, and are expected to attend all the sessions of the academy and to undertake any work which the members of their class may lay upon them. Secretaries are paid twice as much and persons employed for a longer or shorter time for special service are paid as the academy may direct. The chemist, the botanist and the geologist receive a salary which will enable them to live in Berlin.

In 1897, in addition to the publication of memoirs prepared by the members of the academy and contained in the regular 'Proceedings,' and to the making of grants such as it had long been in the habit of making to aid individuals to publish or complete important works of their own, the academy found itself in a position to look toward enterprises which would call for large sums of money and for the labor of many years. To some of them brief reference may be made. It had already taken part in the founding and directing of the work of the Imperial German Archeological Society. The academy is also represented by three of its members, one of whom must be the presiding officer, on the Central Direction of the Monumenta Germaniæ. As has been suggested, it helped to bring into existence the geodetic and meteorological institutes of Prussia and agreed to furnish its share of the cost of the new Latin and Egyptian dictionaries. It has already published complete and worthy editions of the works of Frederick the Great, Luther and Kant, in addition to those of specialists on subjects to which they had given years of labor. In 1888 it was instrumental in founding the Historical Institute in Rome, over which von Sybel presided for five years. This society has a directing secretary who lives in Rome, and is assisted in his work by two competent historical scholars, each of whom is permitted to have a helper. One of its first objects was to collect all the correspondence between the Roman Curia and the nuncios sent to Germany during the Reformation. Five volumes of this correspondence, with two other volumes ready for the press, had been published in 1899. This work has now been brought into affiliation with the Royal Archives, where it will be within the reach of all scholars. Efforts were made in 1893 to gather the papal decrees on all subjects brought before the Curia which concern Germany. These are to be carefully arranged and classified and will go back to the thirteenth century. Work began with the decrees of the first half of the fifteenth century. These decrees are found in seven special Roman archives. The government appropriated at first 60,000 Marks a year ($15,000) for four years, and has since repeated the grant. Beginning with 1897 the director of the institute has edited and published a magazine whose title, 'Sources out of Italian Libraries and Archives,' indicates its purpose and its value. Not only does the academy mediate between the Geodetic Institute at Potsdam and the government, it performs the same service for the Meteorological Institute, which is in close touch with the Royal Observatory for Astrophysics. Upon the collection of Latin inscriptions, of which Mommsen was editor till his death, more than $100,000 have been expended, all of which was obtained through the academy chiefly from the sovereign. On this work, which is nearly completed, Mr. Hirschfield has since 1885 been associated with Mommsen. In 1888 a commission

was appointed for the study of numismatics. It turned its attention first to the work of collecting the ancient coins of northern Greece. Mommsen so greatly appreciated this effort that he turned over to the academy the 28,000 Marks ($7,000) given him on the fiftieth anniversary of his professorship. Vol. I. appeared in 1898. In 1897 two volumes of a work on 'The Posography of the Time of the Roman Emperors' appeared. Mommsen suggested it and was chairman of a commission having it in charge.

The Fronto Commission, seeking to solve a problem proposed by Niebuhr, undertook the publication of the Theodosian Codex and with



F. E. D. SCHLEIERMACHER.

the proceeds of the Savigny fund the publication of what is called a 'Vocabularium juris prudentiæ Romanæ' has been begun. Between 1878 and 1888 a complete collection of Attic inscriptions was published, and in 1899, under the direction of Mr. Frakel, the printing of a treatise on the inscriptions found in the Peloponessus was started, and preparations were made for the printing of those of the islands, those of Lesbos, Nesos and Tenedos being ready at that time. On this work of gathering inscriptions, editing them and giving them to the world, the academy has been engaged more than eighty years. In 1891

it was decided by the academy that an edition worthy the respect of scholars of the Greek writers prior to Eusebius and apart from those whose works relate to the New Testament was greatly needed. The Vienna academy had published the Latin writings prior to the seventh century. The commission into whose hands the work was given in 1893 consisted of Mommsen, Diels, von Wilamowitz-Moellendorf, von Gebhardt, Loofs and Harnack. The cost of this publication will be met jointly from the income of the Frau Heckmann foundation and by the publishing house of J. Hinrich, Leipzig. It is confidently anticipated that in some of these writings questions will be answered like these:

B. G. NIEBUHR.

'How did the church free herself from her early Palestinian society?' 'How did the Roman system arise?' 'How were Greek and Roman culture changed into Greek and Roman Christianity?' 'How did the church receive the middle age culture?' 'Why do the Greek and Roman churches look back to the fathers as their founders, and to these early periods as their classical periods?'

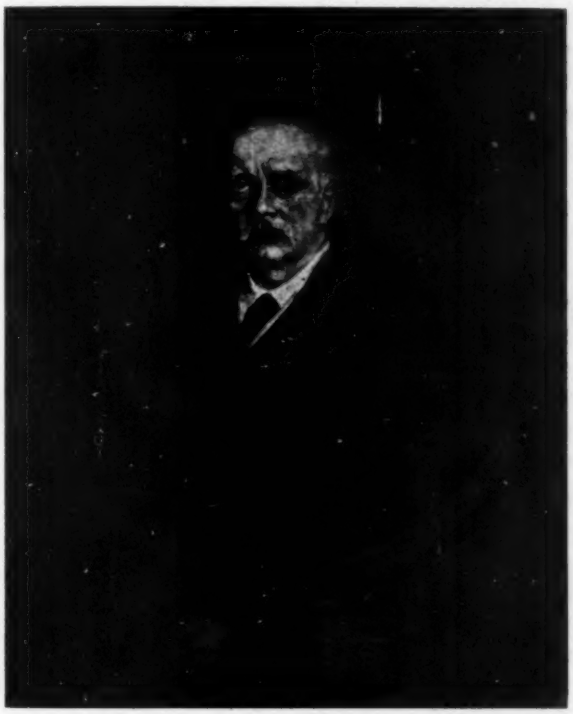
It was a special gift from Frederick William IV. which led the academy to edit and publish the works of Frederick the Great. Droysen

and Duncker proposed (1) that the state and fugitive writings of the king during the first decade of his reign should be collected and printed and (2) that there should be a supplement to the regular edition of his works. It was decided that first of all the political correspondence should appear. Brought down to 1765, this has filled twenty-five volumes, and it is not yet completed. The state writings from the beginning of the reign to the beginning of the Seven Years' War fill three volumes. Out of this undertaking has grown another, that of publishing the 'Acta Borussica,' through which the full history of the Prussian government during the eighteenth century will be given. As early

THEODOR MOMMSEN

as 1899 volumes I. and II. had appeared. When complete the work can not fail to be of immense value to every one interested in the development of Prussia. Researches relating to the history of Brandenburg and Prussia accompany the 'Acta.' These researches are carried on under the direction of the cultus minister and the academy. They have already resulted in the formation of a new school of Prussian historians. The income of the Frau Heckmann gift has rendered it possible to publish a scientific dictionary of the terms used in old

German law. Under the auspices of the academy the works of Jacobi, Dirrichlet, Steiner, Weierstrass and Kronecker will soon see the light. A comprehensive work, taking in the entire animal world, valuable for the unity of its plan and its accuracy has been prepared by Schulze. The income of the Humboldt fund has been expended for the most part on costly journeys undertaken for scientific purposes. Thus Hansel was absent in South America from 1863 to 1867, studying the pampas of Argentina, exploring the bone caves of southern Brazil and observing the remains of mammals. Schweinfurth, the botanist, devoted himself, first at his own cost, as early as 1863, to the study of the



HERMANN VON HELMHOLTZ.

flora of the Nile valley. He went as far as the borders of Abyssinia and the Soudan. On his second journey in 1868, for which he received aid from the academy, he explored Lake El Ghasel, the region round about Njam Njam and Monbutta botanically, geologically and anthropomorphically. He returned to Berlin in 1871 and the conclusions he formed from his studies on his travels are found in the 'Proceedings' of the academy for the years 1870–72. Buchholz, the zoologist, went to equatorial Africa in 1874, and sent home a vast amount

of valuable material for future study. In 1876 Hildebrandt went from Zanzibar to Kilimandjaro and Ndur Kenia; Sachs visited Venezuela in order to study the habits of electrical fishes, and the results of his studies were published by Du Bois Reymond and Nitsch. Fritsch in 1878 was sent to Micronesia to study the rapidly vanishing native races and gather as many as possible of the memorials of their habits and customs. He spent a year in Jaluit, visited several of the smaller islands, then went to New Zealand and New Guinea, and in 1882 brought back to Berlin many thousands of the specimens he had been sent out to obtain. In 1883 Guessfeldt went to the Andes, Arning to Hawaii, and the next year Schweinfurth was sent to examine the desert between the Nile and the Red Sea and report its geodetic and geographical conditions. In 1889 nearly 25,000 Marks were voted to Hensen in aid of an expedition he was preparing to send to Rio Janeiro for the study of sea life. Several naturalists accompanied him. He discovered what he called *planktons,* from which, according to a report made to the academy in 1890, sea life is sustained. To this expedition the king gave 70,000 Marks and other private gifts brought the amount up to 105,000 Marks. Between the years 1890 and 1898, Völkens was sent to Kilimandjaro to study botany; the zoologist von Voelzhow to Madagascar, and Platte, in the interest of the same science, to the coasts of Chili; Fritsch to New Zealand; the geologist Moericke to the Chilian Andes, and the geographer Dove to Africa.

Thus the academy has kept itself in close touch with all recent movements in science, as well as with the advance in literary or historical studies. It has not hesitated to begin work which must take a generation to finish, and of which few of its living members can hope to see the results. Intimately connected with the universities, many of its members, professors in the University of Berlin, enjoying the respect and favor of the reigning sovereign, embracing in its ranks some of the foremost men in science, philosophy and history now living, it has naturally become a center around which the best men of Germany have gathered, and to which the eyes of students, wherever they live, are constantly turning.

# SHORTER ARTICLES AND DISCUSSION.

## *ALUMNA'S CHILDREN AGAIN.*

An article in the May number of the Popular Science Monthly, entitled 'Alumna's Children,' was recently called to my attention by a woman who, though not a college graduate, is 'decidedly a schooled woman' and the mother of five girls. "I had planned," she said, "to let all my girls go to college, but I *do* want to be a grandmother some time."

For answer I heaped the anxious lady's lap with photographs, photographs of babies, babies large and small, babies masculine and feminine, asleep and awake, clean and dirty, elegantly dressed and not dressed at all, but every one a baby to exult over and all the children of my college friends.

Far be it from me to dispute the Massachusetts vital statistics, and farther yet to dissent from 'Alumna's' conclusion that girls in the 'larva stage' need an intelligent care which too few of them receive. But it is not that admirably sane and practical conclusion, nor yet the irrefutable official statistics forming the author's premise that strikes dismay to the mother of five and produces even in those less directly interested an uncomfortable impression of things being dreadfully wrong somewhere. No, it is that dismal array of tragic incidents drawn from the author's personal knowledge, and her consequent theory of causes for the officially vouched for 1.8. It seems only fair, then, to admit to consideration the personal experience of another alumna, an alumna of slightly later date, who may, from that very fact, be able to bring to the matter a slightly different point of view.

I, too, have known of just such brave struggles against physical odds as 'Alumna' reports. I, too, have known of heartrending defeat and dearly bought victory. But the women who have suffered them are not college women. Possibly my experience in this line with my college frends has been an exceptionally happy one, but in granting that possibility we must grant likewise that 'Alumna's' may have been exceptional as well. It is fair, therefore, to balance one against the other. I do not wonder that the mother of five was troubled by Alumna's article. It is obviously deeply sincere and genuinely thoughtful. But when I had read it I glanced up at the photograph of one of the sweetest, sanest mothers who ever presided wisely over the destinies of children, which shows her sitting on one end of a sea-saw with her baby in her lap, smiling up at four little redheads ranged in ascending scale at the other end of the board. Certainly neither college nor preparation for it has robbed of their dues her ten years of married life.

Naturally at this date I can tell of few such families, for most of the college women I know are younger than this one. It is only a few years since my graduation, and three quarters of the class are still unmarried. But all except a few predestined spinsters are still well on the youthful side of thirty, and the percentage of married members is likely to be considerably raised in the next ten years. And of those who are married not only in my own class, but, with a single exception, among my other college friends as well, not one has failed to bear a healthy child within two years of her marriage.

Naturally it is of my own class that I think first, the class whose average scholarship is the highest on the rec-

ords of our *alma mater*, the class who did not use each other's christian names in the freshman year and never walked the halls in embracing couples, incurring thereby a reputation for utter lack of sentiment. But the bond that held us was all the stronger for not being flaunted abroad, and the children of the married members are all 'our babies.'

I put down the magazine and thought of our 'class baby,' our first born, with her splendid, sturdy little body and equally sturdy and independent mind. Not only her mother but her grandmother as well is a product—and a notable one—of the higher education, yet our class baby already possesses a baby brother, two years younger than herself and equally a model of physical and mental health.

Then came a picture of another of 'our babies,' 'the adorable,' with his sunny locks, his starry eyes and his gleesome laugh, always on tap. I thought of his mother as I used to see her crossing the campus, with her fine, high-bred face, her superb carriage and the movement that not even modern draperies could disguise, the very lines of bouyant grace which the 'Winged Victory' has made so familiar.

Then my thoughts strayed to our 'new baby,' the little daughter born in the west with two philosophers for parents and fair 'Mistress Wisdom' herself for godmother. The mother writes: 'My nurse says that health like mine is a thing to be conceited over,' and I know she will meet all the problems of wifehood and motherhood with the same serene clearsightedness that earned her college nickname of 'the Philosopher.'

Two others among my pictures I must mention, amateur photographs both, of sleeping babies. T— was about a year old when that was taken —tousled curls crushed on the pillow, lashes sweeping the rounded cheeks, soft pouting lips, bare dimpled arm and sleep-curled fingers—sweet tranquility and health breathing from every line of the relaxed little figure.

The other is the picture of a wee girl, just a week old. Her mother was the best biologist among the undergraduates of her college. This tiny maid and her sister two years older have spent all their long summers, both before and after birth, in a secluded camp on an Adirondack river shore and something of the woodland influence has entered into their being. They are as shy as young partridges—and as near to nature.

Now the question naturally arises whether the difference between my experience and that of 'alumna' is an accident, or whether it can be explained by a consistent theory. One suggests itself to me which may or may not be correct, that the difference is due to the different *kind* of girl who is going to college now. Only a short decade ago women's colleges drew practically all their students from what might be called the abnormally intellectual class. The girl who went to college, whether rich or poor, whether struggling to escape the demands of society life, or to scrape together money enough for her tuition, was the girl who made matters intellectual of paramount importance and was ready to sacrifice for them, from the grammar school up. College was either an outlet for insatiable mental activity or a technical preparation for the teacher. To girls of that sort domestic life was not imperatively attractive, a fact which may have some bearing on the low marriage rate. And when a woman of this type did marry she was too apt to furnish just such a woeful example as those cited in 'Alumna's' article.

It was the author of 'Harvard Stories,' I believe, who aptly classified all students, as 'grinds, sports—and just boys.' The 'just girl' was for a long time in the minority at women's colleges, but happily she is no longer so. On the contrary she is rapidly securing an overwhelming majority. 'Just girl' she is, 'just woman' she

will be, and the four years of college life is beginning to assume its proper place in public estimation. To produce, not phenomenal scholars nor well-equipped teachers, but fine, strong, human women—that is the function of those precious four years.

Again I turn to my own class—and as I run down the familiar roll from B to W and glance back over the nine years that have made those names part of my life, I see that somewhere, somehow, among the jumble of 'prescribed' and 'elective' courses, we learned therewith the better things, to see largely, to judge temperately, to choose true values. They look but chilly infinitives, written so, but the class knows how they have wrought into the very fiber of our lives and made us the women that we are. And more and more, as the true function of college life becomes recognized, as popular expectation ceases to demand in justification of a B.A. anything but 'just woman,' the type which couples intellectual attainment with underdeveloped body will disappear. For some years the importance of proper attention to the physical well-being of school children of both sexes has been impressing itself upon the public, and no one will apply scientific principles to the nurture of her children more intelligently and with less danger of capricious 'fads' than the college bred mother. We do 'want more' of alumnæ's children, and we are going to get them—an efficient and cumulative force toward those wide and beneficent ends which all true culture stands for.

ANOTHER ALUMNA.

# THE PROGRESS OF SCIENCE.

## *THE SANITARIAN AND THE POPULAR SCIENCE MONTHLY.*

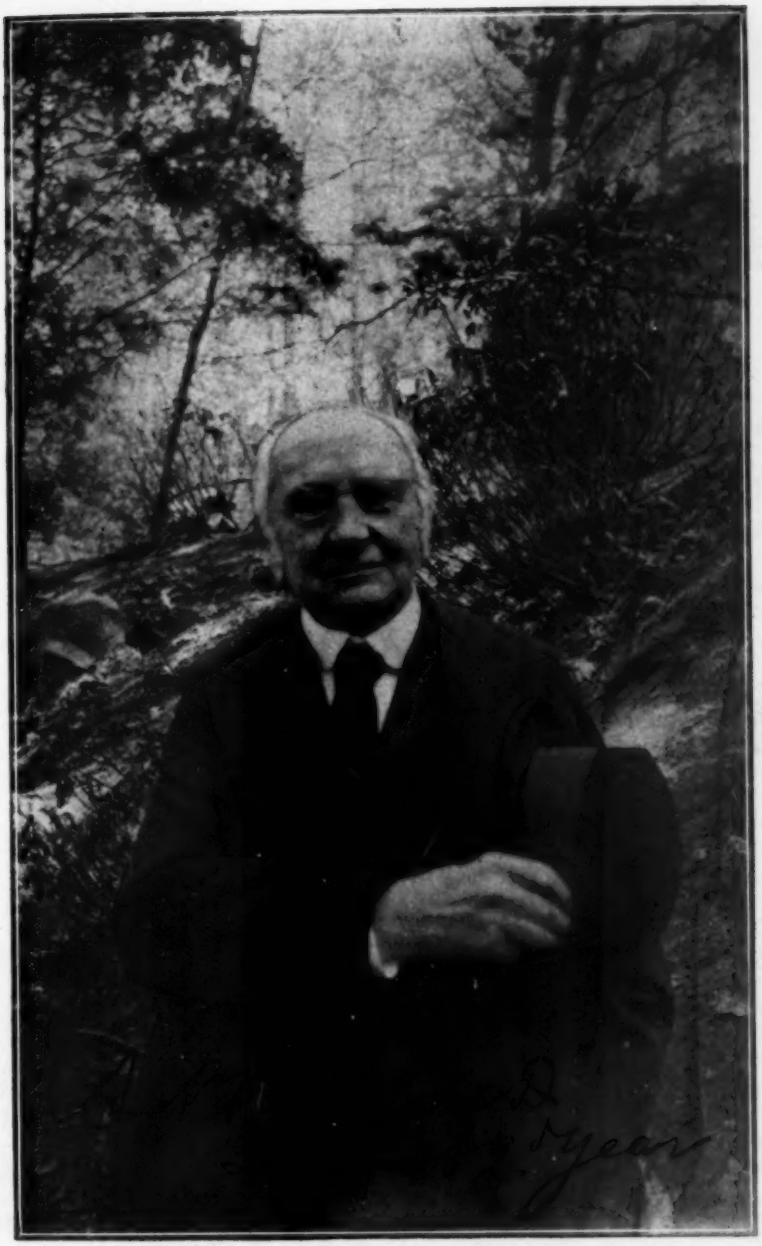
THE *Sanitarian*, established in 1873, and THE POPULAR SCIENCE MONTHLY, established in 1872, are the two oldest journals in English devoted to the diffusion and popularization of science. When the *Sanitarian* was founded there was no journal occupied with sanitary science and public health, and the attention paid to these subjects was comparatively small. The *Sanitarian* has witnessed and promoted one of the most important movements of the nineteenth century. There is more accomplished now for the prevention of disease than for the cure of disease. The 'germ theory' and other discoveries of modern science have led to the forging of weapons more powerful than those used in any other warfare. The mortality of infancy and childhood has been reduced to one half. Infection and contagion are subject to control. The plague, malaria, yellow fever, even consumption, have lost their mysterious terror. We know their causes and can set bounds to their ravages.

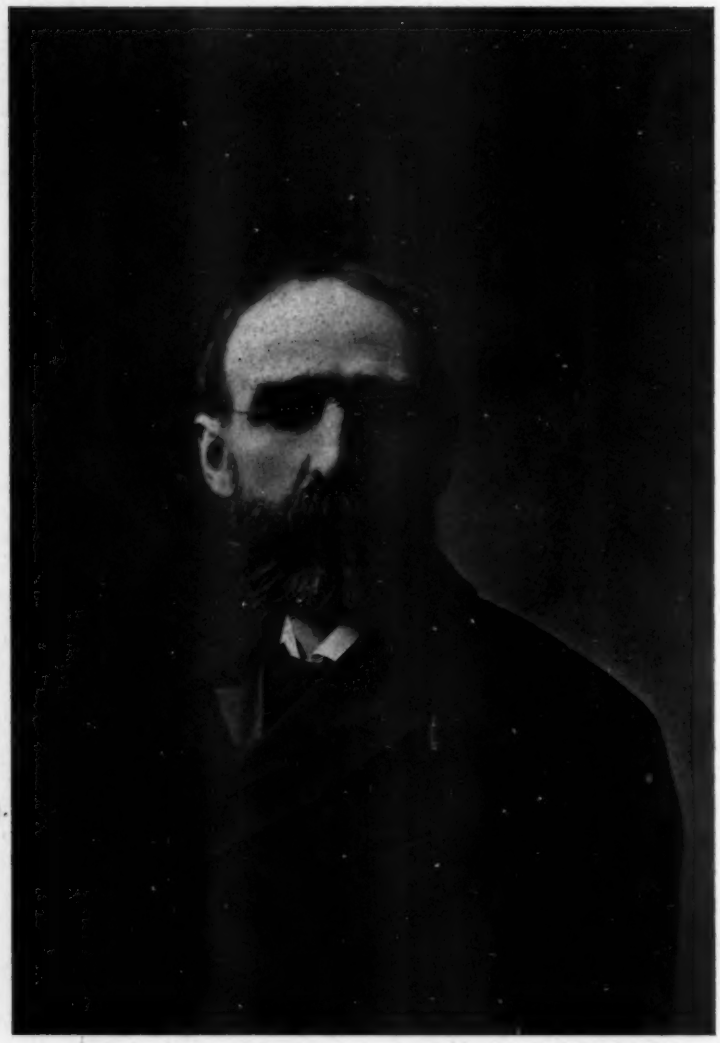
The *Sanitarian* was established by Dr. A. N. Bell, and has been conducted by him for thirty-one years. Dr. Bell is now in his eighty-fourth year, and though vigorous in mind and body, he has earned the right to rest from the labor of conducting a monthly journal. As there are now in the country numerous medical and other journals which give adequate attention to sanitary service and preventive medicine, it has seemed to Dr. Bell that the cause which he has served can be best advanced by merging the *Sanitarian* with THE POPULAR SCIENCE MONTHLY. While the MONTHLY is concerned with all the sciences, it has always aimed to pay special attention to the important field of preventive medicine, and should do so more effectively in the future with the support of the editor, contributors and readers of the *Sanitarian*.

Dr. Bell has assured his reputation not only by the fifty-two volumes of the *Sanitarian*, but also by many other services tending to promote the health of the people. He was born in Virginia on August 3, 1820, and studied at the Harvard Medical School and the Jefferson Medical College. He became surgeon in the navy in 1847, and by services in the Gulf of Mexico, the West Indies and the coast of Africa he became familiar with yellow fever and other diseases. In 1847 he first used steam as a disinfectant, and he early urged the view that yellow fever is not directly contagious, thus greatly simplifying and improving quarantine regulations. As physician, as quarantine officer, as author and as editor, Dr. Bell has earned the gratitude and esteem of all who are interested in the health and welfare of the community.

## *THE JUBILEE OF THE UNIVERSITY OF WISCONSIN.*

THE University of Wisconsin celebrated during the second week in June its fiftieth anniversary and at the same time President Van Hise was formally installed. Michigan, California and Wisconsin form a group of universities which almost rank with Harvard, Yale and Columbia. The great state universities are indeed the more distinctively American, and it is quite possible that the future belongs to them. Millionaires may become less liberal in their gifts, whereas the people of a state are likely to take increasing





CHARLES R. VAN HISE,
President of the University of Wisconsin.

pride in their university. As the alumni become prominent in public life the relations between the state and the university will be even more intimate. So long as a state university deserves support by the direct appeal it makes to the people there is no limit to its development. The milk-test invented by Professor Babcock, of the University of Wisconsin, saves the people of the state annually more than their university costs them. As soon as it is generally understood that a university is an asset, not a charge, our state universities may become the greatest existing centers for education and research.

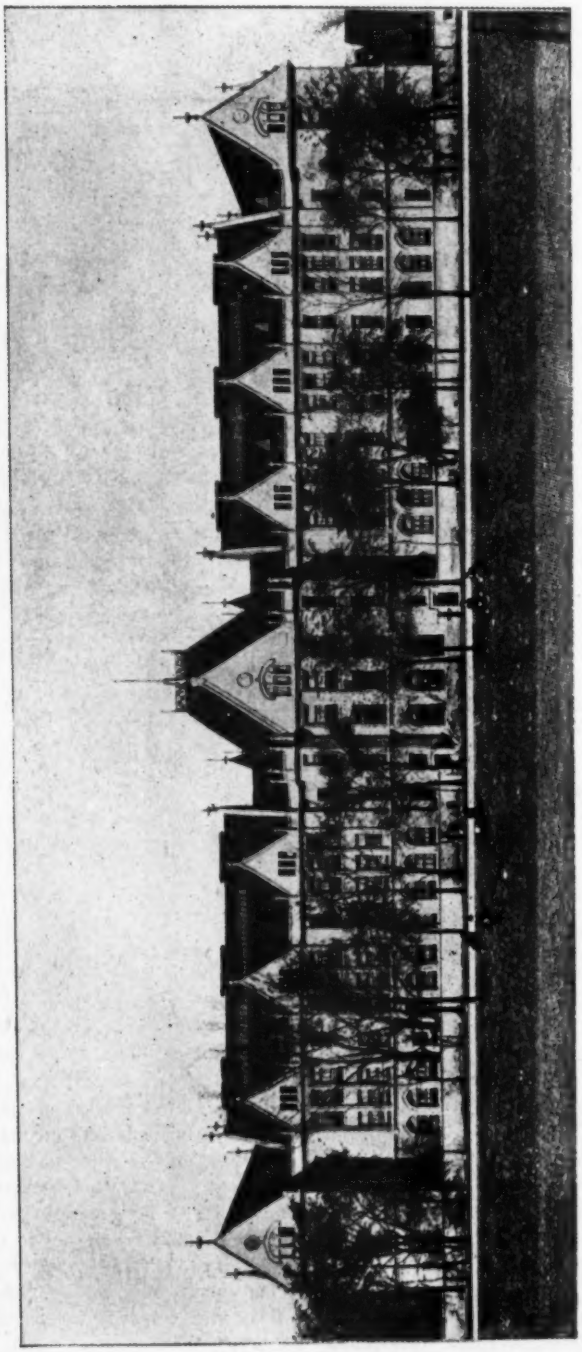
The University of Wisconsin is as a

matter of fact more than fifty years old. In 1838 an act was passed by the territorial legislature establishing the University of the Territory of Wisconsin. Practically nothing was done until the state was organized in 1848, when the university was established by the constitution. In 1866 the university was reorganized by act of the legislature, which also provided for uniting with the university the College of Agriculture, endowed with the proceeds of the agricultural college grant given by the United States in 1862. In 1867 the first annual appropriation (about $7,000) was made by the state. This appropriation has been gradually increased to about $300,000. The state has also provided a great group of fifteen or twenty buildings which are beautifully placed on the shore of Lake Mendota. The library building, used also by the State Historical Society and erected at a cost of over $600,000, is the finest academic building of the kind except that of Columbia. The students number over 3,000, about as many as Yale, Oxford or Leipzig.

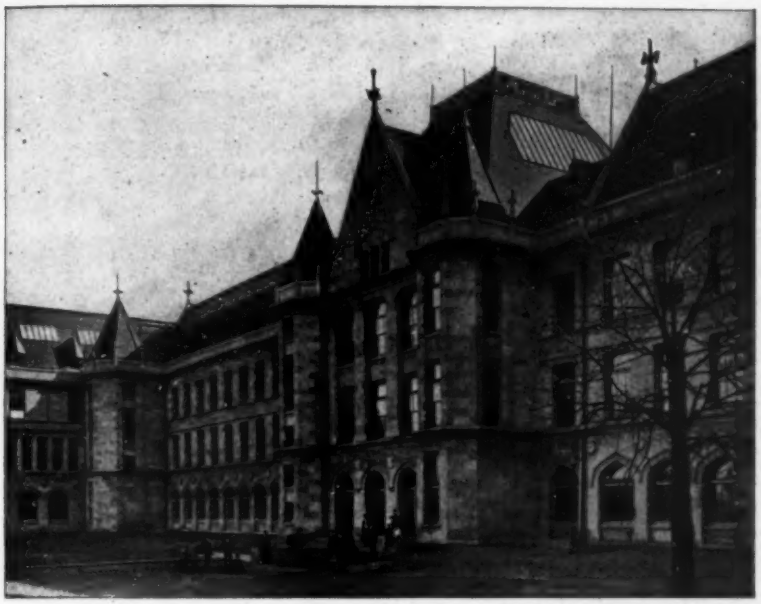
Over this great university one of its own graduates and professors will henceforth preside. Dr. Charles Richard Van Hise was born in Wisconsin in 1857 and has been connected with the university since he entered as a student nearly thirty years ago. He is one of the most eminent American geologists, in charge of the pre-Cambrian and metamorphic geology for the U. S. Geological Survey and a member of the National Academy of Sciences. Some years since when Columbia and Pennsylvania elected business men to the presidency, it looked as though scholarship might be subordinated to wealth and commercial success in this office. But Stanford chose a zoologist, Chicago a Semitic scholar, Yale an economist, Princeton a historian, California a student of Greek, Johns Hopkins a chemist and Columbia a student of education. Now Wisconsin has selected a man of science, one whose interests and character are far removed from politics or commercialism, an ideal scholar.

## *THE CHICAGO SCHOOL OF EDUCATION.*

THE Emmons Blaine Hall of the School of Education of the University of Chicago was dedicated on May 14. This was an event of importance in the history of education and the extension of scientific methods, for it is a step toward making teaching a profession and education an applied science. The University of Chicago has now established a school of education ranking with the professional schools of law and medicine, the first school of this kind being the Teachers College of Columbia University. There are other schools of education and nearly all the larger universities have established departments of education, but Columbia and Chicago are the universities that are leading the way. The two schools had beginnings somewhat analogous. In both cases independent movements for manual training and the less formal education of children have been subsequently taken over by the universities and made into professional schools for teachers with model schools for children attached. Dr. Nicholas Murray Butler, now president of Columbia University, was the first president and practically the founder of Teachers College, now the Department of Education of Columbia University, and it is especially appropriate that he should have given the oration at the dedication of the Emmons Blaine Hall. Other addresses were made by President Harper, of the University of Chicago; Dr. Jackman, dean of the College of Education; Mr. Bentley, of the Chicago Institute trustees; President Downing, of the New York City Normal College, representing the normal schools of the country; Mrs. Blaine, the donor of the hall, and Professor Dewey, director of the School of Education and head of the Department of Philosophy and



EMMONS BLAINE HALL, SCHOOL OF EDUCATION, AS SEEN FROM THE UNIVERSITY.



EMMONS BLAINE HALL FROM SCAMMON COURT.

Education of the university. There were also additional addresses and departmental conferences in connection with the dedication of the building.

In their addresses President Harper and Dr. Jackman traced the history of the school, the former concluding with these words: "And so it has come about that in each case two agencies have united with each other; and that finally all six have been drawn together. These were Colonel Francis W. Parker with his faculty, and joined with them the sympathy and interest of Mrs. Emmons Blaine; the work of the Chicago Manual Training School under Mr. Belfield, and with it that of the South Side Academy, developed under the leadership of Mr. Owen; and, finally, the creative work of Mr. Dewey in his Laboratory School, and in connection with this the factor represented by the university itself. The history of these several movements and of their union with one another has been one of peculiar interest. Many difficulties have presented themselves from time to time, but one by one these difficulties have disappeared. What this school, made up thus of many elements, shall in the end contribute to the cause of education no man can predict. We may hope, however, that the results will be in proportion to the earnest effort thus far put forth by the many who have had at heart the sacredness of the cause. In so far as the school shall represent true ideals, it will help on the work. No more than this could be expected; no more than this could be asked for. The names of Colonel Parker, Mrs. Emmons Blaine, Mr. Belfield, Mr. Owen and Mr. Dewey are written in large letters on the foundation stones of this new structure."

Education and philosophy at Chicago suffer a serious loss by the removal of Professor Dewey to Columbia University. But the work that he has accomplished at Chicago remains; it has sufficient vitality to create its own leaders.

## SCIENTIFIC ITEMS.

WE record with regret the death of Professor E. J. Marey, the eminent French physiologist; of Dr. Wilhelm Hiss, professor of anatomy at Leipzig; of Mr. Robert McLachlan, the well-known British entomologist; of Dr. George Johnston Allman, professor of mathematics in Queen's College, Galway; of Wilhelm von Siemens, the German electrical engineer, and of Professor William Henry Pettee, professor of mineralogy, economic geology and mining at the Universty of Michigan.

AT the jubilee celebrations of the University of Wisconsin the degree of Doctor of Laws was conferred on a number of delegates, including Henry Prentiss Armsby, director of the Pennsylvania Agricultural Experiment Station; Thomas C. Chamberlin, professor of geology, Unversity of Chicago; Professor W. G. Farlow, professor of botany, Harvard University; Daniel Coit Gilman, president of Carnegie Institution; the Hon. James Wilson, secretary of agriculture; Robert S. Woodward, dean of the faculty of pure science, Columbia University; F. P. Mall, professor of anatomy, Johns Hopkins University; E. L. Mark, professor of anatomy, Harvard University, and S. L. Penfield, professor of mineralogy, Yale University.

PROFESSOR CHARLES S. HOWE was inaugurated as president of Case School of Applied Science, at Cleveland, Ohio, on May 11. President Ira Remsen, of Johns Hopkins University, spoke on behalf of the universities; President H. S. Pritchett, of the Massachusetts Institute of Technology, on behalf of the technical schools; John R. Freeman, of the American Society of Mechanical Engineers, on behalf of the technical societies; and President Charles Franklin Thwing, of Western Reserve University, on behalf of the colleges of Ohio. President Howe's inaugural address followed. Mr. John D. Rockefeller has given the Case School $200,000 to be used for building and equipping laboratories for physics and mining engineering.

COLUMBIA UNIVERSITY has conferred its doctorate of science on Professor Hugo de Vries, the eminent botanist of the University of Amsterdam, whose work on the origin of species is described in the present number of the MONTHLY.—The American Academy of Arts and Sciences has awarded the Rumford medal to Professor E. F. Nichols, of Columbia University, for his researches on radiation.—The Chemical Society of London has elected as foreign members Professor E. W. Morley, of the Western Reserve University; and Professor F. W. Clarke, of the U. S. Geological Survey.

THE new medical laboratories of the University of Pennsylvania, erected at a cost of $700,000, were dedicated on June 10.—The New York legislature has appropriated $250,000 for the erection of a building for the College of Agriculture at Cornell University.—The main building of the Rensselaer Polytechnic Institute, Troy, N. Y., was destroyed by fire on June 9.—The corporation of the Massachusetts Institute of Technology voted that the executive committee ascertain whether any arrangement can be made with Harvard University for a combination of effort in technical education such as will substantially preserve the organization, control, traditions and name of the Massachusetts Institute of Technology.